Dr. JOSEPH BLACK.

LECTURES

ON THE

Elements of Chemistry,

DELIVERED

IN THE UNIVERSITY OF EDINBURGH;

BY THE LATE

JOSEPH BLACK, M. D.

PROFESSOR OF CHEMISTRY IN THAT UNIVERSITY,

PHYSICIAN TO HIS MAJESTY FOR SCOTLAND, MEMBER OF THE ROYAL
SOCIETY OF EDINBURGH, OF THE ROYAL ACADEMY OF SCIENCES
AT PARIS, AND THE IMPERIAL ACADEMY OF SCI-
ENCES AT ST. PETERSBURGH.

PUBLISHED FROM HIS MANUSCRIPTS,

BY

JOHN ROBISON, LLD.

PROFESSOR OF NATURAL PHILOSOPHY IN THE UNIVER-
SITY OF EDINBURGH.

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VOL. I.

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....
1807.

CHARACTER OF BLACK'S LECTURES.

"THE name of Dr. Black will probably be remembered as long as
the science of Chemistry exists. His two great discoveries of latent heat,
and of the cause of that difference observable between the properties of
the mild alkalies and alkaline earths, and of these substances, when in a
caustic state, must be acknowledged, by all philosophers, as having com-
municated the impulse, and pointed out the way to the splendid investi-
gations of modern Chemistry. These claims on the remembrance of
posterity, could never have been set aside, even if the present publication
had not taken place. Still it is in a high degree satisfactory to possess a
record of them in the words of their author....more especially as we by
this means become acquainted with the manner, and may form some faint
idea of the effect produced by the lecures of this celebrated teacher.
Professor Robison, the former pupil and intimate friend of Dr. Black,
was entrusted, by his executors, with the arduous and delicate office of
revising the loose manuscript notes, from which Dr. Black delivered his
lectures, and reducing them to a state proper for publication. The docu-
ments of Dr. Black's fame could not have been committed to abler hands.
The volumes before us exhibit a very accurate representation, not only
of the opinions, but, we doubt not, of the very words of the author; while
the notes, which the editor has supplied, from the stores of his own know-
ledge, confer an additional value on the work.

" A large, we will not say a disproportionate share of the work, is
devoted to the illustration of the author's own immortal discoveries,
which are related with great minuteness, and in a most engaging man-
ner. And it is particularly satisfactory to behold on all occasions a most
happy exemption from jealousy of his fellow-labourers in the inexhausti-
ble mine of experimental knowledge, and the most scrupulous equity in
assigning the fame of great discoveries to the rightful claimants."

Aikin's Annual Review, and History of Literature for 1803 *p.* 924.

" In editing these posthumous labours of one of the greatest philoso-
phers which this country ever produced, professor Robison has con-
ferred on the scientific world a most valuable obligation. The lectures
of Dr. Black, as a system of chemical instruction, possess very peculiar
merits—and may fairly be admitted to contain the most accessible stores
of information which persons ignorant of the science can at present com-
mand. They are delivered as much as possible in the analytical mode.
They take for granted no previous acquaintance with science in the
learner—and they require less perhaps than any other work, the assis-
tance of apparatus.

" The two grand discoveries of Dr. Black are those of latent heat,
and the nature of alkaline earths and fixed air. The present publication
contains the only history we have of the former, and a more copious ac-
count of the latter than that which the author published during his life.

" Mr. Robison's notes confer great additional value on this publica-
tion. They illustrate the history of Dr. Black's discoveries, and con-
tain discussions upon various points of modern chemistry of the greatest
importance." *Monthly Magazine, vol.* 16, *p.* 623.

" In bringing forward the present work, Dr. Robison has perform-
ed an acceptable service to the world, and an agreeable and honourable
duty to the memory of an illustrious friend, whose good opinion and con-
degree. To do justice to his reputation as

A

a discoverer, to make known the amiable qualities of his mind, and to give a faithful transcript of those lectures which so much contributed to the advancement of Chemical science, are the laudable views by which the editor was actuated in preparing these volumes for the press. We venerate the undertaking: and we feel much satisfaction in reflecting that the character and labours of one of the most distinguished, and, at the same time, one of the most modest and unassuming philosophers of the last century, are now laid before the public, in a manner which is equally creditable to his memory, and to the feelings, industry and learning of the editor." *Monthly Review, vol.* 42, *p.* 187.

" We introduce this work with peculiar satisfaction; and would recommend it, with an earnestness arising from a complete knowledge of its merits, to every philosophic inquirer. It is an admirable picture of scientific research, according to the plan proposed by Bacon, followed by Newton, and admitted by the best modern enquirers. The inductive reasoning is rigorously strict, thepremises correctly stated, and the conclusions drawn with peculiar precision.

" In this work we have a connected system of both ancient and modern Chemistry, in a form and with advantages far superior to those which any other work has offered

" Some apology may be necesary for our extended account of this work. The character of Dr. Black must furnish this apology. And when our readers reflect that the first germ of the modern system of Chemistry was animated by his labours and ingenuity; that with a degree of judgment equalled only by his acuteness, the first hints were pursued with a vigour of inductive reasoning without a parallel or an example, except in the optics, and without a copy but in the labours of Cavendish and Kirwan—the apology will be easily accepted."

Critical Review, Series Third, vol. 1, *p.* 86 & 284.

" It gives us peculiar pleasure o view the publication of these lectures—and still more to find them edited by a person who enjoyed the personal friendship of Dr. Black, and whose genius is under the control of the same degree of philosophical caution as that of the author. We here behold a veteran professor of distinguished talents, who may be justly styled the father of modern ciemistry, uninfluenced by the supercilious dogmatism of the French scrool. We see him adding the discoveries of the moderns to the original structure of the older chemists, and carefully avoiding those alterations which were only the offspring of vanity, or of the systematic confusion which was introduced by the empirical politicians of that time." *British Critic, vol.* 23, *p.* 645.

The high estimation in which we hold the Lectures of Dr. Black, induces us cordially to recommend them to the friends of chemical science.

Declining any invidious discussions or comparisons respecting the discoveries of their illustrious author, with some fancifully ascribed to earlier chemists, or arrogated by his contemporaries—We believe that lucid arrangement, strength of argument, and excellence of experimental illustration, render them more instructive to the student and more gratifying to the proficient than most other publications in that department of physical enquiry.

JAMES REYNOLDS, M. D.
ADAM SEYBERT, M. D.
B. S. BARTON, M. D.

Philadelphia, *Dec.* 13th, 1806.

Fig. 6.

Fig. 1.
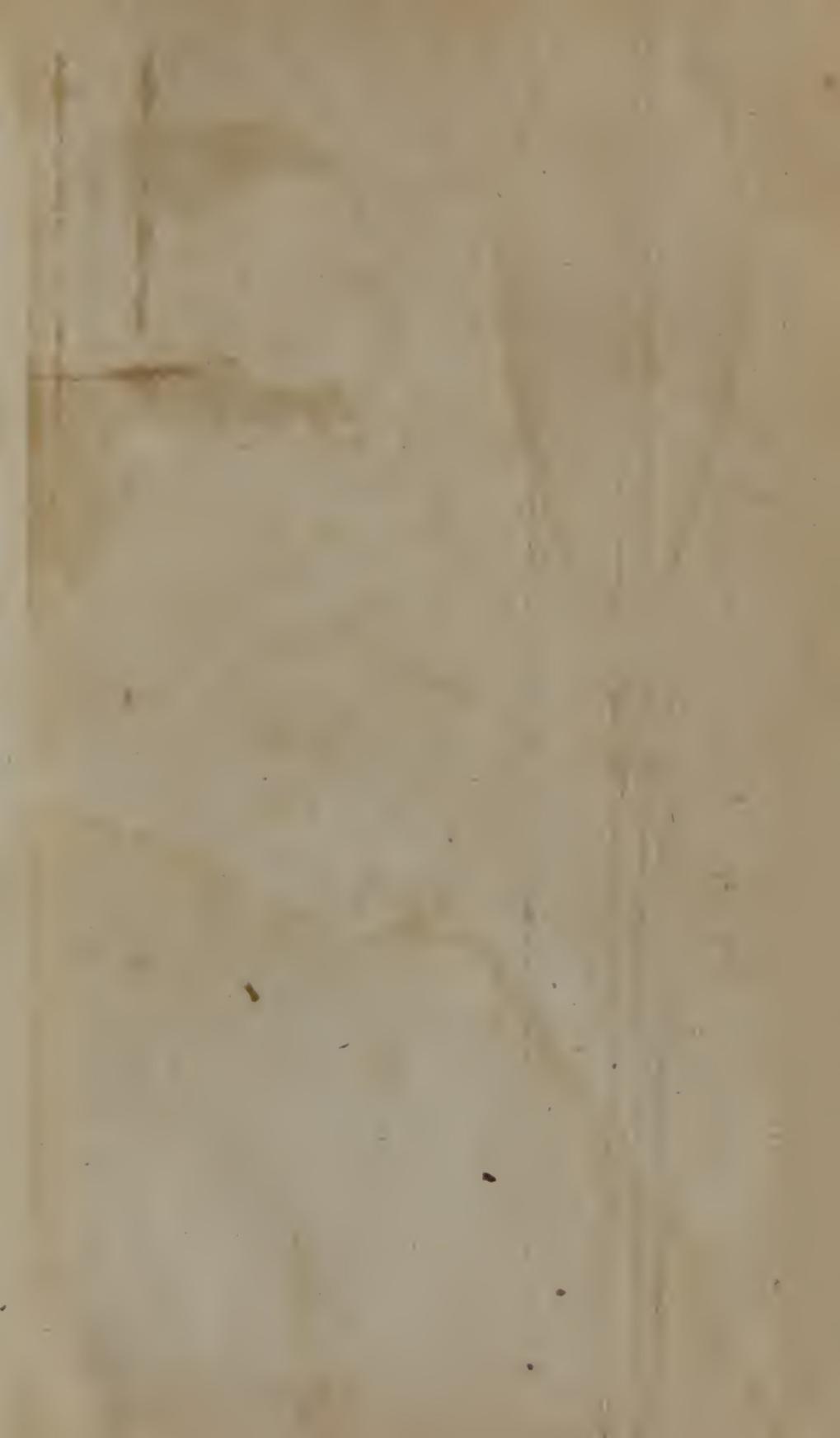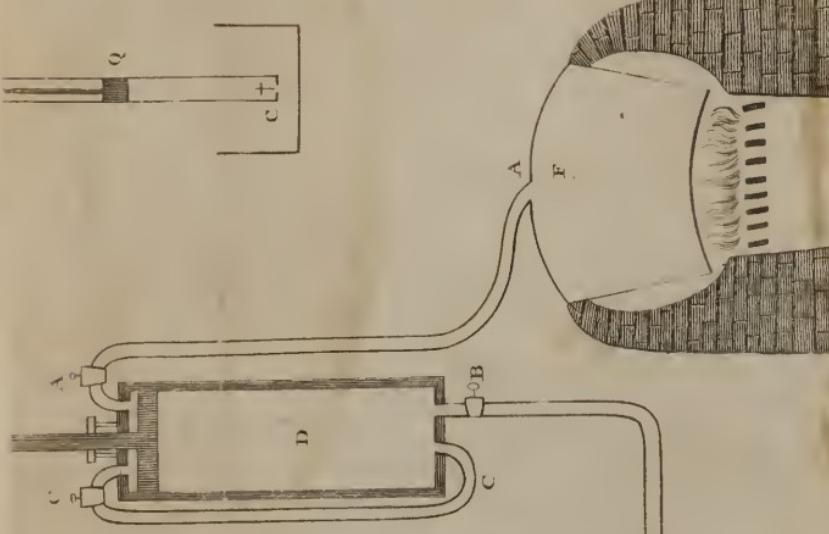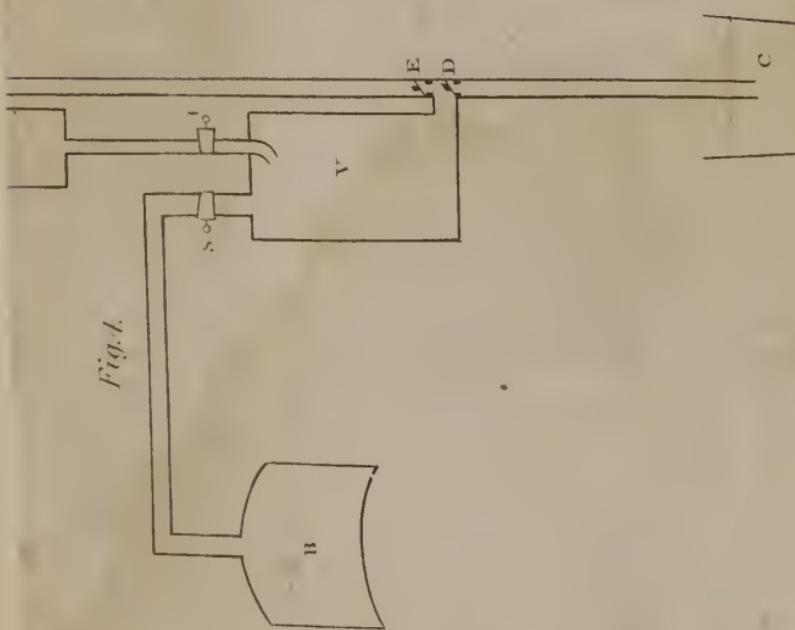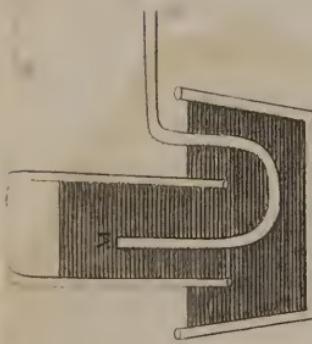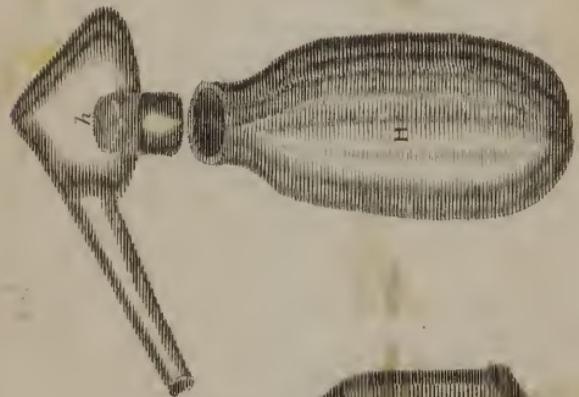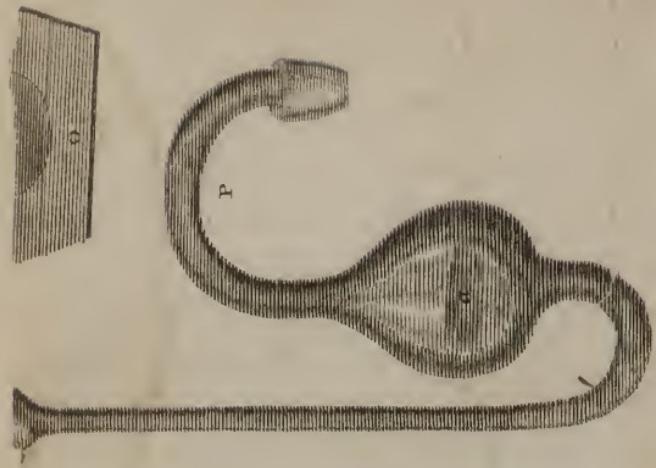
EXPLANATION OF THE PLATES.

A few figures being added to those expressly alluded to in the Lectures, the following short explanation of them was thought necessary.

PLATE II.

Fig. A represents a solution glass; the long taper shape of which permits agitation of the mixture, without risk of spilling: it also prevents the loss of small drops which are sometimes thrown out by effervescence, which would in some nice cases of assay, &c. derange the calculations.

B, with the vessel B below it, is the old circulating apparatus. The ends of the spouts, which come from each side of the capital B, are inserted and luted into the two short tubes projecting from the body or cucurbit B.

C is the Hessian, and D the Ipsian or black-lead crucible.

E is the ordinary form of a retort, of glass, or earthen ware.

F represents a tubulated, or stoppered retort, with its stopper *f*.

G is a separatory, for pouring off the clear part of a solution, or mixture, which has any feculent matter floating above.

Fɪɢ. H represents a cucurbit, with its capital *h*. Its form is very nearly that of the common still. To promote the condensation, the capital should be of a considerable diameter. Observe that the outer rim of the capital should be lower than the hole h in the middle or throat; otherwise, the vapour which condenses on the roof, and trickles down its inside slope, would fall back again into the body H. But the rim being lower, the condensed fluid collects there, and from thence goes into the spout, and runs down into the receiver. The form of a capital is not unlike a mushroom. Observe also, that the throat of the capital should go into the neck of the body, and not encompass it. If the neck fits into the throat, the condensed fluid lies on the luting of the joint, and is contaminated by it.

I is the most approved shape of a receiver. A taper spout *i* is sometimes joined to the remote end, to be inserted into the neck of another receiver, when the surface of one is not deemed sufficient for a speedy condensation. The spout is sometimes joined laterally, as at *i*; by which the fluid obtained by condensation runs off into another vessel, which may be speedily changed when any change of product is expected.

L represents one of the intermediate vessels of Woulfe's distilling apparatus. The tube l reaches nearly to the bottom. The other tube m opens into the space above the water in this vessel, while its other extremity dips almost to the bottom of the next vessel, &c.

M is a sketch of the essential parts of what is called the *pneumatic apparatus*. The tube a, being properly bent, introduces the gas into the water or mercury which filled the jar M inverted into water or mercury. The gas rises up through the fluid, and occupies the upper part b, the fluid subsiding a little on the entrance of every bubble of gas. Its elasticity does not balance the pressure of the atmosphere, till the surface of the fluid within the jar is on the same level with the surface in the cistern c.

N is a perspective view of a muffle. The projections over each of the lateral apertures, are for preventing the coals

Fig. A
No. 3.

No. 2.
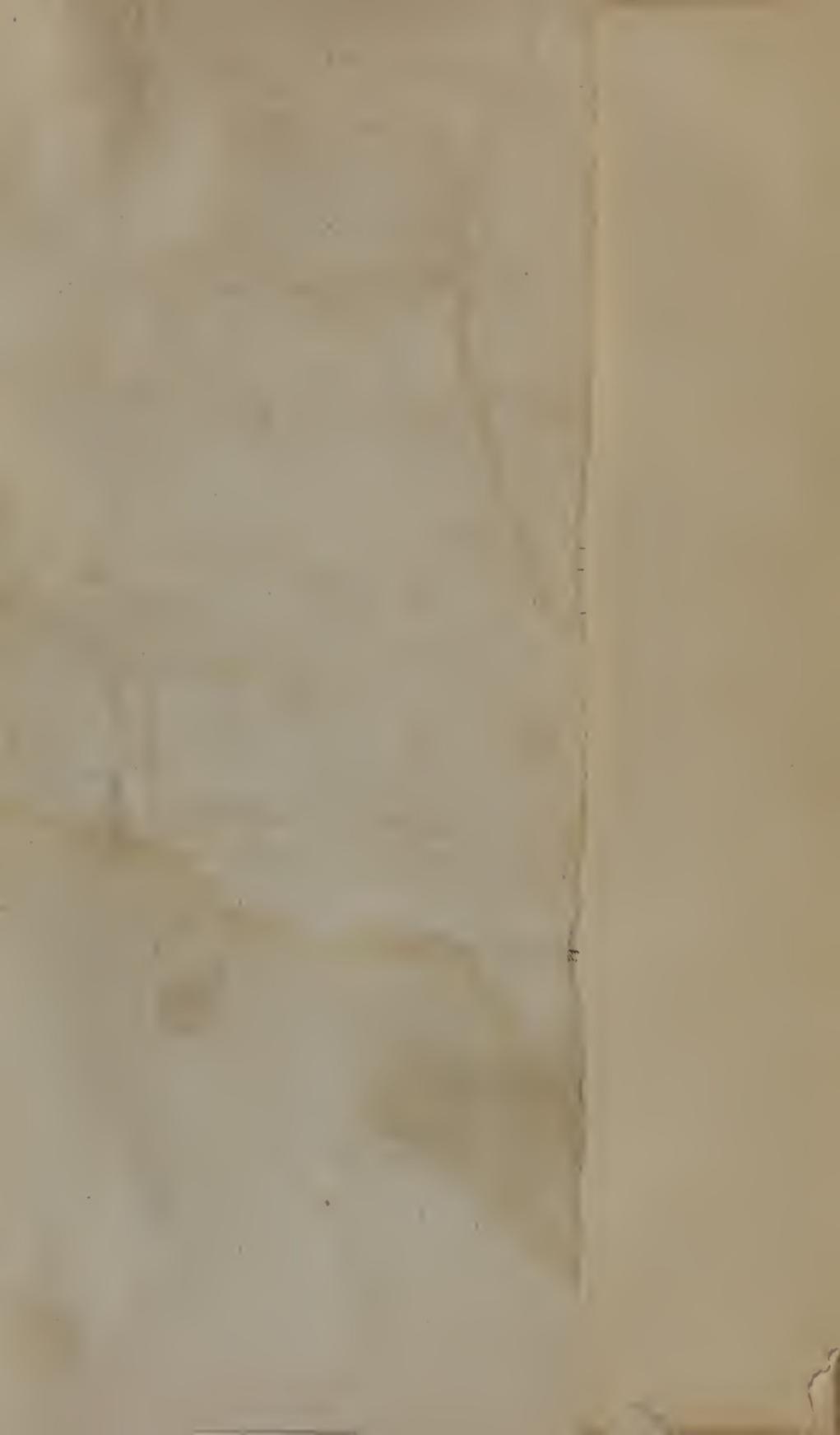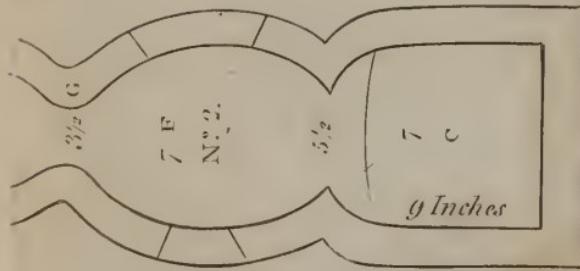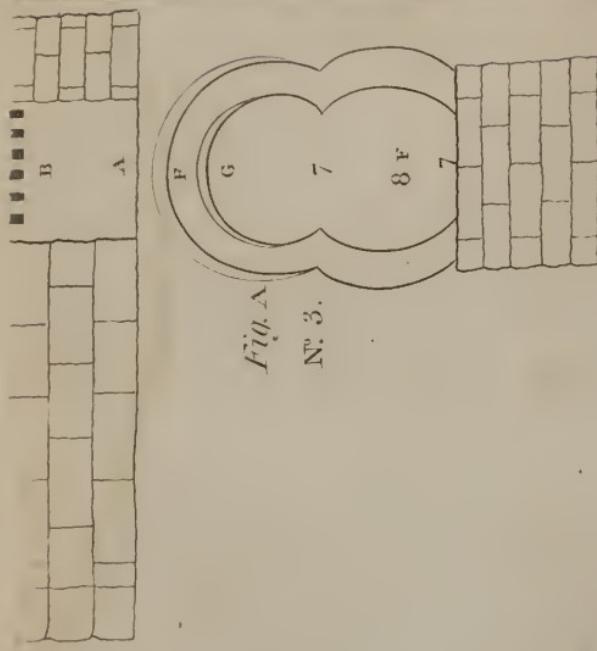
9 Inches

2 feet 10 Inches

with which it is surrounded from tumbling into the muffle
as the fuel subsides.

Fig. O is the section of a cupel made of bone-ashes.

P represents a safety pipe, for distillations, in which the
vapours frequently change their elasticity, and are some-
times reabsorbed by the matter in the retort. One end is
ground like a stopper; and fits a hole in the top of the re-
tort, or other convenient part of the apparatus. When the
vapours tend to burst the vessels, they press on the surface
a of the fluid contained in the ball, and cause it to rise
through the upright tube into the cup at the top,—thus act-
ing against its weight. Should the elasticity still increase,
the gas escapes, by rising in bubbles through the fluid. That
this may always happen, it is necessary that the capacity of
the cup be sufficient to hold all the liquor, which, at the be-
ginning, filled about one-half of the ball below. Then all
risk of bursting the vessels is avoided; and yet a conti-
nual waste of vapour by the vent hole commonly used, is
not incurred.

On the other hand, when a reabsorption commences in
the retort, which would sometimes bring back cold matter
from the receiver, and instantly split the retort, the atmos-
phere will press on the fluid in the vertical pipe, and rise in
bubbles into the ball, from which it will pass into the vessels,
without risk, except in cases where vapours are produced,
which will take fire when mixed with air.

PLATE III.

The only figure in this plate that is not explained in the course of
Lectures is *fig*. A, No. 1, No. 2, No. 3.

They are sections of an experimental furnace by Mr. Macquer,
which is in great estimation at Paris. It is made of pottery, or
earthen ware, set on a base of brick-work.

No. 1. is a longitudinal section along the middle of its breadth. A is
the ash pit, B the ribs of the grate, C the fire place, D its door,
made to lift off and must be very well fitted. E is the throat

of the furnace, through which the flame rushes into the labo-
ratory F, where the arched roof rises considerably. In its
highest part there is a door at the side, represented by the
dotted line. The materials to be scorified, or otherwise treat-
ed in this reverberatory, are put in at this door, and the state
of the process may be seen from time to time. There is a
smaller aperture on the opposite side, even with the floor, or
but a very little higher. The scoriæ are driven into this pas-
sage by the bellows, in certain processes. The arch of the re-
verberatory is much lower at G, where it communicates with
the upright vent or chimney H.

No. 2. is a plan, or rather a horizontal section.

No. 3. is a cross section. F is the fire place: and G is the arch of
the reverberatory.

The numbers on various parts of these three figures are the
dimensions, at those places, in inches.

TO JAMES WATT, ESQ.

OF HEATHFIELD, NEAR BIRMINGHAM.

DEAR SIR,

*BY placing your name in the front of this edition
of the Lectures of our excellent Master, I think that I
pay my best respects to his memory, and also do a service
to the Public. By thus turning the reader's attention to
Dr. Black's most illustrious Pupil, I remind him of the
important services derived from his discoveries: For
surely nothing in modern times has made such an addition
to the power of man as you have done by your improve-
ments on the steam engine, which you profess to owe to
the instructions and information you received from Dr.
Black.*

*When I contemplate the unparalleled state of pros-
perity of the British empire, resulting from the skill,
spirit, and activity of its inhabitants, and reflect on the
imperious call, now upon us, for still greater exertions,
that we may maintain ourselves in this our envied pre-
eminence, I feel it my duty to hold forth every incitement
that can animate to this honorable emulation. I shew
the Reader, in your example, that there is no preemi-*

nence in scientific attainment which he may not hope to reach, by rigidly adhering to the sober plan of experimental inquiry, so constantly inculcated by Dr. Black; and turning a deaf ear to all the fascinating promises of splendid theories. The spark, which I thus throw out, may chance to light among suitable materials,....some felices animæ, quibus hæc cognoscere curæ est,.... minds perhaps unconscious of their own powers. Even yours might have lain dormant, had not Dr. Black discovered its latent fire.

I acknowledge, that, I have also another inducement. I wish to be known as a person who enjoyed a share in the good opinion, and the uninterrupted friendship of two of the most eminent philosophers and most worthy men of the age and nation.

That you may long continue to enjoy your well earned honors, happy in the society of your friends, and still increasing the means of the national prosperity by your inventions, is the earnest wish of

Dear Sir,

Your affectionate Friend,

And obedient humble Servant,

J. ROBISON.

Edinburgh, April 7, 1803.

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THE EDITOR'S PREFACE.

THAT I engaged to revise and prepare for publication the prelections of this eminent Professor, may appear presumptuous, and to require some apology. Chemistry is a science of such immense extent, so multifarious, so abstruse in its principles, and intricate in their combination and mutual dependence, that to pretend to appreciate, or, if necessary, to alter any thing written by Dr. Black, requires no common sagacity, and a degree of information not to be looked for in one who is not professedly a Chemist. And it seems a task too great for any person sufficiently occupied in official duties of a very different nature. I acknowledge the justice of the charge.

But I trust that when the reader, and particularly those who have had the happiness of listening to the prelections of this excellent teacher, is informed in what manner this task fell into my hands, the appearances of unwarrantable presumption will be considerably lessened, and that my endeavors to perform it in a suitable manner will be received with some indulgence.

Immediately after the decease of my worthy preceptor, colleague, and friend, his executors notified their intention of publishing an edition of his lectures from his manuscript notes. This was done in order to prevent a publication, which, they were informed, was intended from notes taken by his students, and gradually improved by corrections and additions of successive years. Copies of such notes were in very general circulation about the College. But the very best of them are so imperfect, have been the work of persons so little acquainted with general learning, and are so full of mistakes, and very inadequate representations of Dr. Black's sentiments, that the executors wished to prevent the unfavorable impression that such a publication would make of the know-

ledge and talents of their departed friend. Mr. Archibald Geddes, manager of the extensive glass-works at Leith, who had long lived in the most intimate and cordial friendship with Dr. Black, and in whose attachment to his memory and reputation the executors had the greatest confidence, mentioned me as a person whom their friend had thought well acquainted with chemistry, and who had been in the habits of intimacy with him in the period of his greatest exertions ; and moreover, as one attached to Dr. Black by every tie of respect, obligation, and affection, and therefore likely to be particularly anxious to support his claims to eminence, by a careful and accurate exhibition of his sentiments and doctrines. This suggestion appeared reasonable, and he was desired to intimate their wish to me.

When the proposal was made to me, it startled me ; but it pleased me. It was very gratifying thus to have the last and the best opportunity of paying my respects to the memory of my excellent friend. I was indeed attached to Dr. Black by every honorable tie. I owed him much ; and the reader will, I hope, forgive me if I mention here the origin of my acquaintance with this celebrated man. It is a piece of self-indulgence, though not foreign to my present purpose, as it shews the early opportunities I enjoyed of acquiring a thorough knowledge of his studies and character.

My acquaintance with him began at Glasgow in 1758, I being then a student in that University ; and it began in a way which marked the distinguished amiableness of his disposition and behaviour. It was at the house of one of the Professors, to whom I was telling the great entertainment I had received from the lectures of Dr. Robert Dick, Professor of Natural Philosophy, and how much I admired him as a lecturer. Dr. Black joined in the commendation, and then, addressing himself to me, questioned me a good deal about Natural Philosophy, so as to perceive what were the peculiar objects of my attention. His advices relative to my favourite study were so impressive, and given in a manner so unaffectedly serious and kind, that they are still as fresh in my mind as if of yesterday's date. I was a stranger to him, and not

even his pupil; and he was prompted to take that pains with me, solely, by the way in which he heard me speaking of the lectures of one whom he loved and esteemed. Gently and gracefully checking my disposition to form theories, he warned me to suspect all theories whatever, pressed on me the necessity of improving in mathematical knowledge, and gave me Newton's Optics to read, advising me to make that book the model of all my studies, and to reject, even without examination, every hypothetical explanation, as a mere waste of time and ingenuity. I am conscious that it was to these advices, so impressively, because so kindly bestowed, that I owe any ability that I may now possess for scientific attainments: For he set me into a path which I fear I should never have chosen for myself.

Our acquaintance was soon interrupted by my leaving college. When I returned to Glasgow, after four years absence, I became Dr. Black's scholar. He was then engaged in his speculations about what he termed *latent heat*. I renewed my acquaintance with him, and with Mr. James Watt, the celebrated engineer. He had already heard two courses of the Professor's lectures, and was completely master of the subject. Dr. Black was pleased to admit me to the greatest intimacy of acquaintance, and few days passed in which I was not in company with these eminent philosophers, both of them in the prime of their scientific energy, and the period of their keenest research. In such society, I must have been stupid not to improve; and this is the time of my life that I recollect with the greatest satisfaction.

When Dr. Black was removed to Edinburgh in 1766, he recommended me to the University of Glasgow as his successor in the chemical lectureship. I exerted my utmost endeavors not to be altogether undeserving of this honor;.... and I am happy to think that these endeavors obtained his approbation, of which he gave me the most convincing proof, by recommending me in the warmest terms to the Patrons of the University of Edinburgh, as well qualified to fill the Professorship of Natural Philosophy, vacant by the death of Mr. Russel, his near relation; a recommendation which, doubt-

less, had ample weight in deciding the competition in my favor. I became Dr. Black's colleague in 1774, and ever since that time have been honoured by his uninterrupted intimacy and friendship. *Si quid est in me ingenii, quod sentio quam sit exiguum, aut si hujusce rei ratio aliqua, ab optimarum artium studiis ac disciplinâ profecta, a quâ ego nullum confiteor ætatis meæ tempus abhorruisse, earum rerum omnium hic Aulus Licinius fructum a me repetere propè suo jure debet. Nam quoad longissimè potest mens mea respicere spatium præteriti temporis, et pueritiæ memoriam recordari ultimam, inde usque repetens, hunc video mihi principem, et ad suscipiendam, et ad ingrediendam rationem horum studiorum extitisse.* **

The proposal, then upon the part of Dr. Black's Trustees and Executors, for charging me with this important duty to his memory, was not without some very reasonable inducements. But every circumstance which rendered this proposal acceptable to me, contributed to point out to me another person, more fit for the task, and more likely to execute it as it ought to be done. This was Mr. Watt of Birmingham :....attached to Dr. Black by the same ties, having had superior opportunities of knowing the whole train of his thoughts, and far more able to do justice to his merits, because profoundly versed in chemical science. I advised this choice, and Mr. Watt was applied to; but his occupations were too incessant, and too serious, to allow him to think of such an avocation.

I had no reason for declining the task, but the very powerful one of bad health, and the fear of its growing worse, and my being thereby rendered unable to fulfil my engagements. It was, however, strongly pressed upon me ; so that, after some fear and hesitation, I agreed to the proposal, on condition of having Mr. Watt's assistance, which the Executors requested and obtained, as far as could be obtained by a correspondence of letters, which Mr. Watt entered into with great cheerfulness and zeal.

But I had not sufficiently weighed the burden which I had taken on my shoulders. I had been informed that Dr. Black had, for two or three years before his death, occupied himself

* Cicero pro Archiâ Poetâ.

in the revisal of the notes of his lectures, and had brought
them into very good order. Two or three of them, which
I looked into, in order to form a judgment of my task, cor-
responded with this account.....and the engagement was en-
tered into. This was in January 1800, while I was occu-
pied with my own college duty, so that I could do nothing in
the affair till the May following. When I then entered se-
riously on the task, I found that the notes were (with the ex-
ception of perhaps a score of lectures) in the same imperfect
condition that they had been in from the beginning, consist-
ing, entirely of single leaves of paper in octavo, full of era-
sions, interlinings, and alterations of every kind; so that, in
many places, it was not very certain which of several notes
was to be chosen. They were often in such a state, that I
could not give them to my amanuensis to be transcribed; and
the only thing that could be done was for me to dictate from
them. I took this method, as the only security for obtaining
a fair transcript. This process necessarily consumed a great
deal of time before I got to the end. It was only then that I
could form a judgment of the performance; for, as I was
going on, almost decyphering, my attention was wholly en-
grossed by the lines before me, and I had scarcely any notion
of a page of it, taken together.

I now found a difficulty of another kind. Throughout the
whole series of Lectures, wherever the subject was very plain
and obvious, the manuscript contained merely a memorandum,
from which Dr. Black had lectured extempore; in many pla-
ces, a reference was made to something standing on the table,
or something going forward in the furnaces. All those blanks
were to be filled up, before I could say that I had made out
even a rough draught of the lectures. This was done, and then
it only remained to make some alterations in the modes of ex-
pression, to cancel allusions to a former day's lecture, and
other circumstances of this kind, which were not suitable to
the appearance in the form of a book. In a few places, I
found myself considerably at a loss to ascertain the author's
meaning, when the reference was very slight, often in a note
with the pencil. I mention all these circumstances, to ac-

count for the seeming delay in the publication. No doubt,
my proceeding was slower than it would have been had I been
in good health; but the additional delay on this account has
not been considerable. I had the assistance of a very fair co-
py of notes, taken by a student, or rather manufactured by the
comparison of many such notes. Copies of this kind were to
be purchased for four or five guineas. This copy belonged to
Dr. Black, and he had made many alterations and insertions
of whole pages with his own hand. It was of considerable
service to me for filling up the blanks above mentioned. Be-
sides the notes which Dr. Black had before him while he lec-
tured, and which were all put into separate parcels, each of
which contained a lecture, there are other small parcels, titled
with the different articles of the course, and containing notes
and memorandums of experiments, quotations from authors,
speculations and conjectures on interesting facts or opinions.
From these also I was frequently enabled to supply what Dr.
Black had said in the lecture.

With such helps, I trust that I have omitted nothing of any
importance, and have every where expressed Dr. Black's sen-
timents with accuracy. This is always done in his own words,
except in the cases already mentioned, where I filled up a
blank in the manuscript. Even in those cases, if the words of
the above mentioned notes taken in the class expressed the
subject with distinctness, I took them, in preference to any in-
sertion of my own, as probably not greatly differing from Dr.
Black's discourse. Where I had no such help, I question
not but that the difference between Dr. Black's manner of ex-
pressing himself, and mine, will be perceived by the gentle-
men who had the pleasure of hearing him. I am sensible
that his language had a perspicuous simplicity which I cannot
attain.

I have twice taken the liberty of making a change in the
arrangement of an article. Dr. Black had considered the *py-
rophori*, such as the *phosphorus of alum*, of *Baldwin*, the *Bol-
ognan stone*, &c. under the same title with the *phosphorus of
urine*, solely, I presume, because they are usually called by the
same name, *phosphorus*. I have placed them at the end of

the article *charcoal*, because they all contain charcoal, and cannot be well understood till the properties of sulphur and of charcoal have been explained. For the same reason, a small part of the account of the *diamond* is separated from the rest, and inserted in the article *charcoal*. The other case is the account of the observations and experiments of M'Bride, Lane, Percival, and those of Priestley, Cavendish, Scheele and Lavoisier, subsequent to the establishment of the theory of quicklime. *(See vol. II. page* 89, &c.) These particulars stood in an order somewhat different from what is followed here; and I trust that the change will be thought to render the account more connected and perspicuous. Indeed, the whole of the article *Alkaline Earths* is not in a form that gave satisfaction to the author; and there lie among these papers several projects for changing both the arrangement and the manner of treating the subject. Dr. Black had always paid too much deference to the futile objections of Professor Meyer, and the partisans of his *acidum pingue;* and had continued to treat those objections as still requiring a refutation, after the theory of *acidum pingue* was altogether exploded. He gives sufficient indication, in these memorandums, of the manner in which he intended to consider the subject; and I should willingly have made the alteration, had I not thought it too great a liberty to insert a composition of my own, in the place of any thing written by Dr. Black.

The memorandums on the medicinal preparations of mercury are so extremely slight and imperfect, that, ignorant as I am of medicine and pharmacy, I could not venture to make any use of them. The memorandums on the chemical analysis of animal and vegitable substances are not in condition fit for publication, not being at all accommodated to the present state of chemical science. Should it appear that this publication is favorably received by the public, an appendix will follow, in the same form, in which these two articles will be properly treated. The doctrine of fermentation (vinous, acetous, and putrefactive) will make a principal article in that work, seeing that they are of the very first importance, both to the artist and to the philosopher.

The discoveries which have been made within these last thirty years, and which have produced so great a change in chemical science, were adopted by Dr. Black, and introduced into his lectures, in proportion as he saw them well supported and confirmed. This, however, taking place by degrees, has occasioned frequent changes in his lectures, and I am not certain that he has been fully decided as to the way in which he should introduce the new doctrines of combustion and acidification. His papers contain several plans to this purpose. One of them, which seems to have had the preference in his estimation, is to bring the whole gradually into view, in the history of the nitric acid, in all its various relations. This method would have been extremely perspicuous and convincing, and had I had sufficient materials of Dr. Black's composition for the beginning of this history, I should perhaps have followed it out, although it would have required almost a new writing of the whole particular doctrines of chemistry. But this not having been supplied to me, I had not sufficient authority for making such a change. As it stands in these volumes, the doctrines are all fully explained, and supported by topics of argument that are quite easy and familiar; and this is done in a manner that gives full confidence. The only deficiency that I perceive is in the incidental way in which the composition of the volatile alkali is introduced; and perhaps also the peculiarities of the oxygenated muriatic acid. Had manganese been first considered merely as a substance affording vital air, the oxygenated form of the acid would have been early established, and this would have afforded solid arguments for some other abstruse points of doctrine.

A discourse, given at the first meeting in each course of lectures, has been omitted entirely, as having little or no connection with their appearance as a book; and a short history of chemistry has also been left out, as too general to be very interesting or instructive.

On the whole, what follows in the text of this work, is the discourse of Dr. Black; as much as possible in his own words, and, in every passage, a faithful exhibition of his sentiments and opinions. I have added some notes, chiefly rela-

tive to the author's peculiar doctrines, and his claims to originality and priority ; sometimes illustrative of the text ; and, on a few occasions, contributing, I hope, to the reader's acquiring just and philosophical notions of the subject. I hope that they will be found pertinent, and, in general, that my endeavours to make the labours of my venerated master useful, as he earnestly wished them, and to appear not unworthy of himself, will be indulgently received by a candid public, to whose judgment they are now submitted with all becoming deference and respect.

When we have received much pleasure or instruction from the writings of any person, we are apt to feel some attachment to the man to whom we have been thus obliged, and some interest in any thing that personally concerns him ;....we wish to be better acquainted with him....to know who he is....what were his usual occupations....his fortunes....his general manners. If we have often listened to him as a public speaker, we cannot help forming to ourselves a notion of his temper and dispositions from the tones of his voice, and the expression of his countenance ; and we figure to ourselves how this person would behave as a companion, a friend, or an inmate. If his public appearance has been conciliating, or eminently pleasing, we wish to converse with him in the familiar intercourse of life to witness him in his domestic occupations, and to share in his amusements. I think that I may venture to say that these are the wishes of all who have listened to this most engaging lecturer....who have remarked the pleasing smile, which began to form on his countenance, when he was about to exhibit or relate any thing that he considered as peculiarly interesting. They will hold themselves as obliged to any man who will bring them more closely into society with this amiable person, by giving some account of his ordinary habits, his studies, his pursuits, and even his fortunes.

I should be happy that it were in my power to satisfy these natural wishes by some discriminating description of Dr. Black, by some account of his life, his character, and manners. This would not only please, but (as I apprehend) would instruct, by exhibiting a fair example of what is amiable and worthy.

But I am too sensible *quid ferre recusent, quid valeant humeri*, to think myself equal to this task....to give such a picture that a competent judge shall say, " This is Dr. Black." I will, however, for the satisfaction of his pupils, and as a sort of duty to my departed friend, mention such circumstances of his life and situation, as have come to my knowledge, especially such as have some relation to this publication of his official discourses. For this purpose, I shall use the freedom to avail myself of the information contained in a paper read to the Royal Society of Edinburgh, (of which Dr. Black was a member), by his near relation, Dr. Adam Ferguson, Professor of Mathematics in the University, and well known in the republic of letters, by works of the very first rank.

Dr. Joseph Black was born in France, on the banks of the Garonne, in the year 1728.* His father, Mr. John Black, was a native of Belfast in Ireland, but of a Scotch family, which had been some time settled there. Mr. Black resided, for the most part at Bordeaux, where he carried on the wine trade. He married a daughter of Mr. Robert Gordon, of the family of Hilhead in Aberdeenshire, who was also engaged in the same trade at Bordeaux, where he was very successful, and had the satisfaction of clearing the family estate in Scotland of the incumbrances which had been increasing for some generations.

The mother of Dr. Black, and the mother of Mr. James Russel, Professor of Natural Philosophy in the University of Edinburgh, were sisters; and the mother of Dr. Adam Ferguson was their aunt, a circumstance which was the origin, though not the cement, of a friendship subsisting between them through life. Their ostensible connection became much closer by Dr. Ferguson's marrying a daughter of Dr. Black's sister, a young lady who had a great deal of her uncle's elegance of mind and manners.

Mr. Black of Bordeaux was a gentleman of the most amiable manners, candid and liberal in his sentiments, and of no

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* In the Inaugural Dissertation, published when he received the degree of Doctor of Medicine, he stiles himself *Galla Hibernus.*

<div align="right">American Editor.</div>

common information. These features of his character, and particularly the strength of his attachments, and the warmth of his heart, appear in the strongest manner, in a series of letters to his son, which have been preserved by him with the nicest care.

So much worth had not escaped the discerning eye of the great Montesquieu, one of the presidents of the court of justice in that province. This illustrious magistrate and excellent man honored Mr. Black with a friendship and intimacy altogether uncommon; of which his descendents are, to this day, justly proud. They preserve letters, and fragments of correspondence, between the president and their ancestor, as they would titles of honour descending in their family. On a paper wrapped round a bundle of such letters, I find the following note in the hand-writing of Dr. Black:

" My father was honored with president Montesquieu's " friendship, on account of his good character and his virtues. " He had no ambition to be very rich, but was cheerful and " contented, benevolent and liberal minded, industrious and " prudent in business, of the strictest probity and honor, " very temperate and regular in his manner of life. He, and " my mother, who was equally domestic, educated thirteen of " their children, eight sons, and five daughters, who all grew " up to be men and women, and settled in different places. " My mother taught her children to read English, there being " no school for that purpose at Bordeaux."

The great partiality of the president Montesquieu for the constitutional government of these kingdoms is well known. I think it not unlikely that he derived much of his information about many things peculiar to our fortunate situation from the British gentlemen whom he might see at Bordeaux. Here he could make his inquiries at his leisure, and here he was certain of being listened to with attention and respect. His stay in England was short and hurried; too many objects were forced on his attention at once. This however was not his chief inducement to an intimate acquaintance with the family of Mr. Black. With a great simplicity of heart and manners, this illustrious person had a glowing sense of modest

worth. His private memorandums, lately published, seem truly to express his genuine sentiments and choice in the enjoyment of life. Though solicited by every thing that could encourage a man of honorable ambition, he quitted the capital in the height of his reputation for professional talents, and even quitted his high office in the province, that he might be fully master of his time, and choose his society. He lived almost constantly at his country seat in the neighbourhood of Bordeaux.

I am sorry that I did not look more narrowly into the President's correspondence with Mr. Black. I can only recollect the being delighted with the impression which the sight of so much worth and domestic happiness made on this excellent judge of human nature. Dr. Ferguson mentions a letter, written by him, when he heard of Mr. Black's intention to leave Bordeaux. In this letter, among other expressions of kindness, are the following: " I cannot reconcile " myself to the thoughts of your leaving Bordeaux. I lose " the most agreeable pleasure that I had, that of seeing you " often, and forgetting myself with you." I remember also another letter, in which were nearly the following words: " I " rejoice to hear of the good health of all your family; and I " endeavour to make your satisfaction solace me for the loss " of those tranquil hours which I enjoyed in the midst of " my friends, contemplating their happiness and their vir- " tues."

I could not refuse myself the pleasure of recording these few particulars, descriptive of an amiable family, which so warmly engaged the notice and the heart of this illustrious friend. But they are not without their use, when they shew the charm of domestic virtue, and the unspeakable advantages of good habits in the outset of life. Mr. Black lived to see that his paternal care had not been in vain.

Long before Mr. Black retired from business, his son Joseph was sent home to Belfast, that he might have the education of a British subject. This was in the year 1740, he being then twelve years of age. After the ordinary instruction in a grammar school, &c. he was sent, in 1746, to continue his

education at college, in the University of Glasgow. I have no account of those youthful studies, but I presume that he had employed his time to good purpose. I infer this, partly, from some passages in his father's letters to him, expressive of his great satisfaction with the accounts which he had received from others, of his progress in his studies ; and partly, from the very 'perspicuous manner in which I have heard Dr. Black state the distinctions between the theories of ethics which had been taught at Glasgow by Dr. Francis Hutcheson, and, after him, by Dr. Adam Smith. Physical science, however, had chiefly attracted his attention; and he was a favorite pupil of the professor of natural philosphy, Dr. Robert Dick, and the intimate companion of his son and successor. This young professor was of a character peculiarly suited to Dr. Black's taste, having the clearest conception and soundest judgment, a manly steadiness of opinions and conduct, accompanied by a modesty that was very uncommon. When he succeeded his father in 1751, he became the delight of his students, and was, indeed, the most perspicuous and instructive lecturer I ever heard. Yet Dr. Black informed me that he was unhappy in the thoughts of not being sufficiently qualified for the office, and wished to resign it. He was carried off by a fever in 1757, Dr. Black always spoke of him in terms of the highest respect for his talents and his great worth, and I could observe that he was always pleased when I made him the subject of conversation. Dr. Dick had been the chosen friend of his youth.

Being required by his father to make choice of a profession, Mr. Black preferred that of medicine, as the most suited to the general habits of his studies, not foreseeing, during the happy gaiety of youth, how much he would suffer by anxious solicitude and fears in the practice of this noble art.

It was fortunate for Dr. Black that, when he began his medical studies at Glasgow, the celebrated Dr. William Cullen had just entered on his great career, was become conscious of his own strength, and saw the unoccupied field of philosophical chemistry open before him. It had been treated hitherto only as a very curious and useful art, which was in-

deed susceptible of much improvement by means of rational
inquiry and discussion. But Cullen saw in it a vast depart-
ment of the science of nature, which must be founded on prin-
ciples as immutable as the laws of mechanism, and which may
be one day formed into a great system of doctrines, of vari-
ous degrees of subordination and dependence. He was deter-
mined to attempt this mighty task, and promised himself
great reputation by its accomplishment. Nor was he alto-
gether disappointed. He quickly succeeded in taking che-
mistry out of the hands of the artists, the metallurgists, and
pharmaceutists, and exhibited it as a liberal science, the study
of a gentleman. He carried into his medical lectures the
same ideas of a great system of nature, and made his pupils
perceive something of that affinity by which, as Cicero finely
observes, *all the sciences are connected, tendering to each other
a mutual illustration and assistance.* His pupils became
zealous chemists as well as refined physiologists. Young
Black was particularly delighted with a view which accorded
so happily with those enlarged habits of thought which he had
acquired ; and his great bias to this study was soon perceived
by Dr. Cullen. No professor took a more lively interest in
the progress of an emulous student than Dr. Cullen. It was
his delight to encourage and assist their efforts, and there-
fore he was not long in attaching Mr. Black to himself, in the
most intimate co-operation ; insomuch, that the latter was
considered as his assistant in all his operations ; and his experi-
ments were frequently adduced in the lecture, as good autho-
rity. Thus began a mutual confidence and friendship, which
did honor both to the professor and his pupil, and was always
mentioned by the latter with gratitude and respect.

Our young philosopher had laid down a very comprehen-
sive and serious plan for the conduct of his studies. This
appears by a number of a note-books found among his pa-
pers. There are some in which he seems to have inserted
every thing as it took his fancy, in medicine, chemistry, juris-
prudence, or matters of taste ; and I find others into which
he has transferred the same things, but has distributed them
according to their scientific connections. In short, he has

kept a journal and leger of his studies, and has posted his books like a merchant. I have looked over these memorandums with some care, and have there seen the first germs of those discoveries which have at last produced such a complete revolution in chemical science. What particularly struck me, was the steadiness with which he advanced in any path of knowledge,....*nulla retrorsum.*✻ Things are inserted for the first time, from some present impression of their singularity or importance, but without any allusion to their connections. When a thing of the same kind is mentioned again, there is generally a reference back to its fellow ; and thus the most insulated facts often acquired a connection which gave them scientific importance.

By these references I got the order in which things had occurred, (for there are very few dates) and a pretty certain knowledge of the years when he made the observations. For, in what I call his day-books, mention is sometimes made of occurrences that were perfectly known to me, as I had lived in the place from my infancy, and was well acquainted with every thing that passed. In the oldest parcels of these notes, I find queries respecting the nature of cooling mixtures, and the cold produced by liquefaction ; but it is not till some time after, I think not before the year 1752, that I can date any observation relative to fixed air. Yet this was the first of his investigations that he completed, by establishing it as a general law of nature. I do not imagine that Mr. Black's researches at this time (or perhaps at any time) have been keen or pertinacious. This could not accord with the native gentleness of his mind ; but his conceptions being distinct, and his judgment sound, his progress in scientific research, if slow, was steady, and his acquisitions were solid. Perhaps this moderation and sobriety of thought was his happiest disposition, and the most conducive to his improvement. I am convinced that it was from experience that he was led to caution his pupils so earnestly to check the first incitements of high expectations, and never allow their fancy to be warmed by the brilliant appearance of some general view, which promised at the first glance to explain a multitude of phenomena.

This calmness of procedure appears in another circum-
stance. The College Registers shew that Mr. Black might
have offered himself as a candidate for a doctor's degree,
near three years earlier than he actually did. When he was
graduated in 1754, he had not availed himself of his medical
standing in the University of Glasgow, but took the course
prescribed by the rules of that University.

Dr. Black went to Edinburgh, to finish his medical studies,
in 1750, or 1751. There he lived with his cousin-german,
Mr. James Russel, Professor of Natural Philosophy in the
University. Mr. Russel was really a philosopher. No man
saw more clearly the great scale of Nature, as it is diversified
by the powers of mechanism, chemical affinity, and the prin-
ciples of growth, life, sentiment, and intellect: and I think he
was the first who ascertained with some precision the true
pale of Natural Philosophy, and gave a truly philosophical
table of its various parts. He was not more enlarged in his
general views, than he was ingenious in his examination of
subordinate parts; and he was most happy in the talent of
presenting things to the mind in a simple and familiar man-
ner. In such a society, Mr. Black must have passed his time
both agreeably and profitably.

It was the good fortune of chemical science, that at this very
time, the opinions of professors were divided concerning the
manner in which certain lithontriptic medicines, and particu-
larly lime-water, acted in alleviating the excruciating pains of
the stone and gravel. The students usually partake of differences
of opinion, and are thereby animated to more serious study, and
science gains by their emulation. This was a subject quite suited
to the taste of young Mr. Black, one of Dr. Cullen's most zea-
lous and intelligent chemical scholars. It was indeed a most
interesting subject, both to the chemist and the physician.
All the medicines which were then in vogue, as solvents of
the calculous concretion, resembled more or less the *lapis in-
fernalis*, and the common ley of the soap boilers, two sub-
stances so terribly acrimonious, that in a very short time,
they will reduce the firmest and most solid parts of the ani-
mal body to a mere pulp. Therefore, while they were powerful

lithontriptics, they were hazardous medicines, if in unskilful hands. All of them seem to derive their efficacy from quicklime, and this derives its power from fire. Its wonderful property of becoming intensely hot, and even sometimes ignited, when moderately wetted with water, had long engaged the attention of the chemists. It was, therefore, very natural for them to ascribe its power to igneous matter imbibed from the fire, retained in the lime, and communicated by it to alkalis, and other substances, which it renders so powerfully acrid. Hence, undoubtedly, arose the denomination of *causticity*, given to the quality so induced. I see that Mr. Black had entertained the opinion, that caustic alkalis acquired igneous matter from quicklime. In one memorandum, he hints at some way of catching this matter as it escapes from lime, while it becomes mild by exposure to the air;....but on the opposite blank page is written, " Nothing escapes,....the cup " rises considerably by absorbing air." A few pages after this, he compares the loss of weight sustained by an ounce of chalk, when calcined, with its loss when dissolved in spirit of salt. Immediately after, a medical case is mentioned, which I know to have occurred in November 1752. From this it would appear, that he had before this time suspected the real nature of these substances. He had then prosecuted his inquiry with vigour; the experiments with magnesia are soon mentioned.

These laid open the whole mystery, as appears by one other memorandum. " When I precipitate lime by a common " alkali, there is no effervescence. The air quits the alkali for " the lime, but it is not lime any longer, but c. c. c. It now " effervesces, which good lime will not." He had now discovered that the terrible acrimony of those powerful substances is their native property, and not any igneous matter derived from the lime, and by the lime from the fire. He had discovered that a cubic inch of marble consisted of about half its weight of pure lime, and as much air as would fill a vessel holding six wine gallons, and that it was rendered tasteless and mild by this addition, in the same manner as oil of vitriol is rendered tasteless and mild in the form of alabaster, by its combination with calcareous earth.

What a multitude of important consequences now present themselves to the mind of an intelligent chemist and physician? I am inclined to think, that it was at this time that the animating hope of scientific reputation first dawned on the mind of this young philosopher. His experiments on the lithrontriptic medicines, by shewing him the true nature of quicklime, had in one glance shewed him what causticity is, and to what substances it belongs, and how to induce it, or remove it, or direct its activity at pleasure. This was a subject even more interesting to the physicians than to the chemist. It had divided the opinions in both departments, and given rise to many mysterious notions concerning the nature of fire (and favorites, because mysterious) derived from the very remarkable properties of quicklime. Our notions are now altogether reversed. Lime imparts nothing; it only removes from substances, naturally caustic, that air which renders them mild; and by this addition it becomes mild or inactive. These mysterious notions are now exploded as mere fancies; and great simplicity is now perceived in those operations of nature, which formerly appeared very intricate and abstruse. And it is evident that the practice of physic must derive unspeakable advantage from all this information.

It is surely a dull mind that will not be animated by such a prospect. I presume that Mr. Black felt its genial influence; and I suppose that having fixed on this for the subject of his inaugural essay, he deferred application for a degree, till his doctrine should be established beyond the possibility of contradiction, by a train of decisive experiments. Thus did an honorable ambition happily accord with his native moderation of character.

The inaugural essay, and the precise time of its appearance in public, were fortunate circumstances for science. At this very time, Dr. Cullen was removed to Edinburgh, and there was a vacancy in the chemical chair at Glasgow. On whom could it be bestowed with so much propriety, as on such an alumnus of the University,....on one who had distinguished himself, both as a chemist, and as an excellent reasoner? For

I hesitate not to say, that excepting the optics of Newton, there is not a finer model for philosophical investigation, than the essay on magnesia and quicklime. He was appointed professor of anatomy and lecturer on chemistry in the University of Glasgow, in 1756. Had this vacancy not happened, it might probably have been the lot of Dr. Black to practise medicine in some provincial town of Britain or Ireland ; and thus occupied, the serious concern which he took in the cases under his treatment would have absorbed his whole attention, and might perhaps have overpowered his feeble constitution. It was therefore equally fortunate for himself and for the public, that a situation now presented itself, which allowed him to dedicate his talents chiefly to the cultivation of chemistry, his favorite science.

Here I must not omit a circumstance told me by his brother. When Dr. Black took his degree in medicine, he sent some copies of his essay to his father in Bordeaux. A copy was given by the old gentleman to his friend the president Montesquieu, who, after a few days, called on Mr. Black and said to him, " Mr. Black, my very good friend, I rejoice with " you; your son will be the honor of your name and of your " family." That sagacious philosopher saw, with the first glance, the door opened to a field of research, altogether novel, and of unknown extent. What could be more singular than to find so subtile a substance as air existing in the form of a hard stone, and its presence accompanied by such a change in the properties of that stone ? What bounds could reasonably be set to the imagination, in supposing that other aëreal fluids, as remarkable in their properties, might exist in a solid form in many other bodies, which at present attract no notice, because of our ignorance of their nature and their composition ? Here was a new instrument put into our hands, and a new mode of investigation suggested ; and it appeared unquestionable that many and great discoveries must be made in this new track of inquiry. indeed, I have often wondered that Dr. Black was not more incited to proceed in this track, which he had so clearly laid open. This must have proceeded from the multiplicity of new duties which crowded on him at once, in con-

sequence of his department in the University of Glasgow. His first appointment was to the professorship of anatomy, and the lectureship on chemistry. He did not consider himself as so well qualified to be useful in the former branch of medical study, but was determined to do his utmost. Soon after, however, he made arrangements with the professor of medicine, and, with the concurrence of the University, the professors exchanged their tasks.

His lectures, therefore, on the Institutes of Medicine, were his chief task. They gave the greatest satisfaction, by their perspicuity and simplicity, and by the cautious moderation of all his general doctrines. It required, however, all this perspicuity, and all this neatness in the manner of exhibiting simple truths, to create a relish for this great moderation and caution, after the brilliant prospects of systematic knowledge, to which the students had been accustomed from the Doctor's celebrated predecessor. But Dr. Black had no wish to form a medical school, which should be distinguished by some all-comprehending doctrine. He contented himself with giving a clear and systematic account of as much of physiology as he thought founded on good principles, and a short sketch of such general doctrines as were maintained by eminent authors, but perhaps on a less firm foundation. Without this, he said that his students could not read their writings, which, in other respects, were highly valuable. He then endeavoured to deduce a few canons of medical practice; and concluded with certain rules, founded on successful practice only, but not so deducible from the previously delivered principles of physiology; observing that we should not despair of being able, on some future day, to proceed in the opposite direction, deducing the first principles entirely from the practice. It does not appear, however, that he had ever satisfied himself with his method of treating those subjects. He did not encourage conversation on those topics; and there are no remains of his medical lectures to be found among his papers. I owe the account now given of them to a respectable surgeon in Glasgow, who attended these lectures in the two last years of his Professorship in that University.

My acquaintance with Dr. Black gave me full opportunity of seeing, from his extreme anxiety about his patients, that he was deeply impressed with a sense of the immense importance of the healing art. I cannot doubt, therefore, but that his thoughts were much and most seriously employed on his medical lectures; and I am confident that the less he was satisfied with the limited portion of knowledge he could communicate to his students, the more earnestly would he strive to increase it.

It is in this way that I account to myself for the remarkable fact, that, although Dr. Black had opened such a new and boundless field of chemical research, which promised so much, both of knowledge and of fame, and in which we see, by the progress of some very slovenly adventurers, that it was extremely easy to discover objects, both new, and wonderful, and important; that notwithstanding all this, he did not immediately engage with ardor and perseverance in this race of discovery and of honor. No doubt, his duty as Professor of Chemistry forced him to divide his attention, and probably other tracks of chemical enquiry might also hold out very tempting objects. But still nothing can more clearly shew Dr. Black's calm and unambitious character.

But all this was very unfortunate, in my opinion, for the world. What a difference there would have been between the patient, judicious, and progressive investigation of Dr. Black, and the hasty, wavering, and often slovenly experimenting of some manufacturers in science, whose wish to get first to market with every thing was represented by them as proceeding from public spirit, while the endeavors of others to correct their own errors, to arrange and methodize their materials, and thus to advance securely, though slowly, in the great path of philosophical discovery, was attributed to a narrow-minded pride, or the selfish vanity of being accounted the author of a system. But, *est modus in rebus*.....It must be owned that Dr. Black was too little animated by his own success,....too insensible to the real value of literary fame, and to the notice taken by the public of his discoveries, and not sufficiently excited to the vigorous prosecution of them.

His theory of quicklime and causticity was soon known to the German chemists, and met with strong opposition. Mysterious doctrines concerning the intimate nature of fire were very prevalent in the German schools, and were very various. Their notions of the causticity of alkaline substances always involved some of these doctrines. This gave rise, therefore, to a variety of objections to a doctrine which took this matter out of the hands of the pyrologists altogether. The most formidable opponent was Dr. Meyer of Osnaburg, who had published a considerable volume on quicklime, in which he professed to explain all the phenomena by the action of *acidum pingue*, formed in the lime during calcination, and consisting of igneous matter in a certain inexplicable combination with other substances. There is scarcely to be seen a book in which there is such a number of injudicious experiments, and unskilful attempts to reason from them. Yet this performance of Professor Meyer gave Dr. Black considerable uneasiness; and, for several years, he was at the pains to refute all his arguments, and all his objections to the theory given by himself, but without adding a single experiment to those by which he had already established it. Yet the obvious and simple experiment, of making the matter expelled from marble by a calcining heat pass into a solution of caustic alkali, and render it effervescent and mild, would have ended all disputes. This was done by Jaquin, at Vienna, in 1767, and at once silenced all the German chemists, as the experiment of Well, in which he calcined marble by a burning glass, put an end to Meyer's theory. But Dr. Black always expressed a dislike to the bringing forward a multitude of experiments of the same kind, saying that he felt his own confidence more forcibly won by one judicious and simple experiment than by any number of examples of inferior evidence.

Dr. Black's reception at Glasgow by the University was in the highest degree encouraging. His former conduct there as a student had not only done him credit in his classes, but had conciliated the affection of the Professors to a very great degree. When he returned to his Alma Mater Academia as a Professor, he was immediately connected in the strictest

friendship with the celebrated Dr. Adam Smith; a friendship which became more and more intimate and confidential through the whole of their lives. A certain simplicity of character, with an incorruptible integrity, which was acutely sensible to the smallest indelicacy or incorrectness, was instantly seen by each of these friends in the character of the other, and riveted the band of their union. Dr. Smith used to say that no man had less nonsense in his head than Dr. Black; and he often acknowledged himself obliged to him for setting him right in his judgment of character, confessing that he himself was apt to form his opinion too generally from a single feature. Indeed, were I to say what intellectual talent Dr. Black possessed in the most uncommon degree, I think I should say that it was his judgment of human character, and a talent which he had of expressing his opinion in a single short phrase, which fixed it in the mind, never to be forgotten. Dr. Smith's pictures of men had not always this precise similitude, he being more apt to decide hastily of character; and he was not unfrequently mistaken in the judgments he formed on a short acquaintance.

When I returned to College in 1763, I found Dr. Black in high reputation as a professor, and a favorite physician of that large and active city. Indeed his sweetness of manner, which the dullest eye must have perceived to be free from all studious endeavor to please, and the evident concern which he took in the cases under his care, could not but make him a most welcome visitor in every family. His countenance, at that time of life, was equally engaging as his manners were attractive, so that I do not wonder that, in the general popularity of his character, he was in particular a favorite with the ladies. I could not but remark that they regarded themselves as honored by the attentions of Dr. Black; for these were not indiscriminately bestowed, but exclusively paid to those who evinced a superiority in mental accomplishments, or propriety of demeanor, and in grace and elegance of manners.

But I am forgetting the professor and philosopher. It was at this time, between the years 1759 and 1763, that he brought to maturity those speculations concerning the combination of

heat or fire with the substance of tangible matter, which had long occupied his thoughts occasionally. The simple experiments and familiar observations by which he demonstrated the fixation (I may call it) of heat in bodies when it melts or evaporates them, render the inference so palpable and obvious, that one is disposed to wonder that it had not been made long before. But it is really not so obvious, and it requires attentive reflection, to conceive distinctly the procedure of nature. When I lift a piece of wood out of some box or vessel, where every thing has been kept extremely cold, I feel it cold in my hand. If I lift out of the same box a piece of lead, it feels colder still; and a piece of ice from the same place feels colder than either. The first suggestion of sense is, that I *receive* cold from the wood;....more from the lead;....and that the ice proves a source of cold till it be all melted. But the person who is habituated to the consideration of things of this nature makes an inference which is precisely the contrary to all this. Such a person infers that the wood *takes* a little heat from his hand, and is soon warmed so much as to take no more. The lead takes more heat from him before it be as much satiated; and the ice continues to rob him of heat as fast as in the first moment, and therefore feels equally cold till all be melted. Dr. Black made this inference. He had also some vague notion that the heat so received by the ice, during its conversion into water, was not lost, but was contained in the water. His chief inducement to think so, was a curious observation of Fahrenheit, recorded by Dr. Boerhaave, namely, that water would sometimes grow considerably colder than melting snow, without freezing, and would freeze in a moment, if disturbed, and in the act of freezing emitted many degrees of heat.

But how was this conjecture to be confirmed or refuted? Dr. Black hoped to do this by comparing the time of raising a pound of water one degree in its temperature, with the time required for melting a pound of ice, both being supposed to receive the heat equally fast. And on the other hand, by comparing the time of depressing the temperature of a pound of water one degree, with the time necessary for freezing it completely, he should learn how much heat emerged during

the congelation. If the conjecture be just, as much heat must be observed to come out of the pound of water in freezing as were lost in melting a pound of ice. This thought occurred to him in the summer season: and as there was no ice-house then in Glasgow, he waited with impatience for the winter; and in December 1761, he made the decisive experiment, in a large hall adjoining to his college rooms, expending on the ice during its liquefaction, and obtaining from the water during its congelation, as much heat as would have raised the water somewhat more than 140 degrees in its temperature.

But I must observe here, that this experiment, so anxiously longed for by Dr. Black, only served to inform him *how much* heat was thus absorbed by the ice, retained by the water while fluid, and emitted by it in the act of freezing. But he had already full conviction of the truth of the doctrine, by reflecting on the observations of every day of frost or thaw. Since a fine winter day of sunshine did not at once clear the hills of snow, nor a frosty night suddenly cover the ponds with a thick cake of ice, Dr. Black was already convinced that much heat was absorbed and fixed in the water which slowly trickled from the wreaths of snow; and on the other hand, that much heat emerged from it while it was as slowly changing into ice. For, during a thaw, a thermometer will always sink when removed from the air into melting snow; and during severe frost, it will rise when plunged into freezing water. Therefore, in the first case, the snow is receiving heat, and in the last, the water is allowing it to emerge again. These were most unquestionable inferences, from observations the most familiar; and they now appear most obvious and easy: yet, before Dr. Black, no person seems to have made them. Fahrenheit, Boerhaave, Mairan, De Luc, and all the inquisitive meteorologists of the two preceding centuries, though incessantly contemplating and employing the same facts in their disquisitions, never mention having had such a thought; nor is a trace of it to be seen in the laborious collections of that unwearied compiler, Professor Muschenbroeck. It is the undivided property of my ingenious and acute preceptor.

Philosophers had long been accustomed to consider the thermometer as the surest means for detecting the presence of heat or fire in bodies, and they distrusted all others. Yet this instrument gave no indication of the presence of these 145 degrees of heat in the water. Dr. Black therefore said that the heat is *concealed* in the water....*latet;* and he briefly expressed this *fact*, by calling it concealed or *latent heat*. The epithet expressed purely and accurately the very circumstance he wished to express, and he could not have pitched on one more proper. Yet even this unexceptionable epithet was sometimes misunderstood; and latent heat was spoken of as something different from other heat. But this proceeded from mere inattention.

Dr. Black having established this discovery in the most incontrovertible manner, by simple and decisive experiments, drew up an account of the whole investigation, and the doctrine which he founded on it, and read it to a literary society which met every Friday in the Faculty room of the college, consisting of the members of the University, and several gentlemen of the city, who had a relish for philosophy and literature. This was done April 23, 1762, as appears by the registers of the society.

Dr. Black quickly perceived the vast importance of this discovery; and took a pleasure in laying before his students a view of the extensive and beneficial effects of this habitude of heat in the economy of nature. He made them remark how by this means there was accumulated, during the summer season, a vast magazine of heat, which, by gradually emerging, during congelation, from the water which covers the face of the earth, serves to temper the deadly cold of winter. Were it not for this quantity of heat, amounting to 145 degrees, which emerges from every particle of water as it freezes, and which diffuses itself through the atmosphere, the sun would no sooner go a few degrees to the south of the equator, than we should feel all the horrors of winter.

His thoughts on this combination of heat were not confined to the simple case of water, but extended to every phenomenon of congelation and liquefaction, not even excepting the changes

which are effected by the functions of animal and vegetable life. He conceived the accretion of solid matter as a source of a part at least of the warmth of animals.

His thoughts running in this manner over every phenomenon of composition and resolution of heat, he had long found reason to suspect the legitimacy of the measures of heat given by the thermometer. He had made experiments on some bodies which change their mode of aggregation from solid to fluid, and the contrary, not all at once, but by imperceptible degrees; and had found that, in the whole of this change, they were absorbing heat. Resin and sealing wax are very clear examples of this. A thermometer plunged into resin in very thin fusion, and compared with another standing in oil in the same vessel, shewed, from the very beginning, a very different progress of refrigeration. We are not certain that something of the same kind does not happen in water and other fluids. They may have different degrees of fluidity in different temperatures, although we have no way of discovering them, so fluid do they seem in all temperatures.

Such were the surmises which made Dr. Black think it necessary to examine the scale of the thermometer, in order to learn whether equal differences of expansion corresponded to equal additions or abstractions of heat. He made a series of experiments on this subject, and read an account of their result in the literary society above mentioned, on the 28th of March, 1760.

The result of this inquiry was, that equal additions or abstractions of heat produced equal variations of bulk in the liquor of the thermometers employed by him, and therefore that the scale of expansion was also a scale of heat. Dr. Black did not know, at that time, that the celebrated mathematician, Dr. Brooke Taylor, author of the *Method of Increments*, had had the same doubts respecting the thermometric scale, and had examined it by the very same experiments.

These surmises and doubts about the truth of the thermometrical indications, arose entirely from the notion which was floating in his mind about *liquefying heat*, and from the partial and incomplete nature of his occasional experiments on

melting and congealing bodies. Much about the same time, Mr. De Luc entertained similar suspicions; but they proceeded from considerations altogether different; from doubts about the equableness of expansion by equal variations of temperature in short, from the same doubts that had occurred long before to Brooke Taylor, and to Rhenaldini. These were the thoughts of a philosopher interested only in an instrument of research. But the result of his scrupulous examination of thermometers was most unexpected and important; for, without this information, naturalists were liable to enormous mistakes in their judgments of temperature. Dr. Black's surmises about the thermometric scale were those of a chemist, studying the nature of fluidity.

There is such an analogy between the cessation of thermometric expansion, during the liquefaction of ice, and during the conversion of water into steam, that Dr. Black had no sooner explained the first of those anomalies, than he felt in his own mind that all his former conjectures about a variety of phenomena in the boiling, and even in the gentle evaporation of fluids, were well founded; and he was persuaded that in the same manner as ice, in liquefaction, requires the combination of a great quantity of heat, in order to form water, so water, in order to its conversion into steam, also requires another combination with heat, in an unknown proportion. When he considered the slow production of steam, notwithstanding the continued heat of glowing fuel in contact with the vessel....the scalding power of steam....and the great heat raised in the refrigeratory of a still....he was so much convinced of the perfect similarity of Nature's procedure in both cases, that he taught this docrine, in his lectures in 1761, before he had made a single experiment on the subject; and he explained with great facility of argument, many phenomena of nature which result from this *vaporific combination of heat.* This must not be considered as unwarrantable or hypothetical. It resulted from a careful study of those facts which the operations of nature continually presented to him. He saw no occasion for more experience for establishing the fundamental proposition. I have some notes taken in the class this

session, by a nobleman eminent for his science and learning, by which it appears that Dr. Black had brought his thoughts on this subject to *full maturity*, and that nothing was wanting but a set of plain experiments, to ascertain the *precise quantity* of heat which was combined in steam, in a state not indicated by a thermometer, and therefore latent, in the same sense that the *liquefying heat* is latent in water.

Whoever thinks seriously on the many interesting objects which chemistry presents on every hand to the man of philosophical curiosity....how many things appear, of which we can give no account, and yet are of extensive influence in many departments of the science....and who reflects on the feelings of an ingenuous and honorable mind, engaged by duty to give instruction on all these particulars....will not wonder that Dr. Black did not immediately support his doctrine by an apparatus of experiments, which should fix every point, and leave nothing to be added by others to a discovery so new, so curious, so important, and so intimately connected with his theory of liquefaction. But Dr. Black was not a trader in science....nor had he any strong incitement from literary ambition, to make him neglect, either his system of lectures, every one of which required his earnest study, or his patients, whose cases at all times filled him with anxiety and solicitude. It was late before he had made such experiments as satisfied him, in respect to the precise quantity of the heat latent in steam....not till the summer of 1764. But he had not rested all this while satisfied with mere conjectures. He thought that what we may observe every day is sufficient for settling the main questions; for we know that the temperature of a fluid rises by every addition of heat, till it begins to boil.... after which it rises no more, let it boil ever so violently. We know that steam, though so powerfully scalding, is no hotter by a thermometer than boiling water. Therefore the heat which enters the water while boiling, is either lost altogether, or is concealed from the thermometer....*latent in the steam*. And lastly, the observed scalding power of steam, and the heat which it imparts to the worm tub of a still, are sufficient proofs that this heat is really contained in the steam, and may

be brought out of it again by reconverting it into boiling hot water. Dr. Black had verified all this, and narrated his experiments in his lectures. The experiments were of the most simple kind, and the inference from them most obvious and satisfactory. He observed that whatever time was employed to heat water from its ordinary temperature (about 50°), to the boiling temperature (212°), the same fire must be applied five times as long, in order to convert it all into steam. Hence he might infer that the steam had carried off 810 degrees of heat. He found, on the other hand, that a pound of water passing along the worm of a still in the form of steam, communicated 20 degrees of heat to 40 pounds of water in the worm tub. Hence he inferred that the steam had given out 800 degrees of heat.

Fortunately for Dr. Black, and for the world, he had now gotten a pupil who was as keenly interested in this scientific question as the Professor. This was Mr. James Watt, then employed in fitting up the instruments in the M'Farlane Observatory of the University; a philosopher, in the most exalted sense of the word, who never could be satisfied with a conjectural knowledge of any subject, and who grudged no labour or study to acquire certainty in his researches. He chanced to have in his hands, for repairs, a model of Newcomen's steam-engine, belonging to the Natural Philosophy Class, and was delighted with the opportunity which this small machine gave him for trying experiments connected with the theory of ebullition, which he had just learned from Dr. Black. These he prosecuted in a most happy train of success, and did not stop, till his steam engine was rendered more like the most docile of animals, than a frame of lifeless matter; so that, while its power is competent to the lifting a house from its place, a child of ten years old shall, with a touch of his hand, make it go fast or slow, forwards or backwards, and act either powerfully or feebly. This gentleman attached to Dr. Black by every tie of respect, esteem, and affection, supplied him with proofs and illustrations in abundance, of all the points on which the professor wanted information. These were always recited in the class, with the most

cordial acknowledgement of obligation to Mr. Watt. Mr. Irvine also, a young student of medicine, of a quick apprehension, sound understanding, and particularly disposed to consider every thing mathematically, was at this time a hearer of Dr. Black's lectures, and greatly captivated with chemical science. He engaged, with great pleasure and zeal, in all examinations which seemed to interest the Professor, and particularly such as would admit of mathematical consideration; thermometrical experiments on the scale of heat;....on the connection between expansion and variation of temperature; ....on the measures of heat, &c.;....all these were fields of research altogether to his mind. He supplied Dr. Black with a a vast number of experiments on the equilibrium of heat, on the specific heats of different substances, and on the continued absorption and fixation of heat by glass, sealing-wax, resin, and other substances, which gradually become more fluid. The register of these experiments are in my possession, and are similar to those which now fill many pages of the Memoirs of the foreign Academies. I think it my duty now to call upon his son, who, I am informed, inherits much of his father's philosophical spirit and ingenuity, to look over his papers, and see whether any of them have been put into a state fit for public view, being confident that the studies of such a man as Dr. Irvine must have been extremely ingenious and important.

It was with Mr. Irvine's assistance that Dr. Black made his first experiments for measuring the latent heat of steam. He found it to be as much as would raise seven or eight hundred times as much water, one degree in its temperature by Fahrenheit's scale; which fact Dr. Black expressed by saying that steam contained 700 or 800 degrees of heat latent in it. Afterwards, with the assistance of Mr. Watt, and better apparatus, he found that the latent heat of steam was not less than 850, and sometimes very considerably exceeded this, being so much the more as the pressure of the atmosphere diminished.

Thus was established another law of nature, of most extensive and important influence in the train of changes that are going on around us. Here we observe another combination

of heat or fire, the mighty agent by whose operation all these
changes are affected. Heat, or the cause of heat, seems now
to put on a real form, and is no longer liable to be considered
as a mere condition or state, into which other matter may be
brought; as noise or sound is known to indicate merely a cer-
tain undulating or tremulous motion of air, or other elastic
matter. But we now see heat susceptible of fixation, of being
accumulated in bodies, and, as it were, laid by, till we have
occasion for it; and we are as certain of getting the stored-up
heat out of the steam or the water, by changing them into
water or ice, as we are certain of getting out of our drawers
the things we laid up in them.

The influence of this last combination of heat is much more
extensive than appears in the experiments by which its reality
was established. Dr. Black discovered that this absorption and
accumulation takes place, not only in the boiling of all fluids,
and all conversions of matter into strongly elastic steams, but
also in every case of evaporation, even the most gentle and
unperceived. When the hand is dipped into warm water,
and then held up in the air till the film of water adhering to
it is dried off, we feel it remarkably colder than the other hand,
exposed to the same air. If we dip one finger into a glass of
water, and another of the same hand into a glass of strong
spirit of wine, and hold up the hand in the air, the finger dip-
ped into the spirit is the first dry, and till it be dry, it feels
remarkably colder than the other; but now, the other con-
tinues the colder of the two, till it also be perfectly dry.

Here we can also trace another magnificent train of changes,
which are nicely accommodated to the wants of the inhabit-
ants of this globe. In the equatorial regions, the oppressive
heat of the sun is prevented from a destructive accumulation
by copious evaporation. The waters, stored with their vapo-
rific heat, are thus carried aloft into the atmosphere, till the
rarest of the vapour reaches the very cold regions of the air,
which immediately forms a small portion of it into a fleecy
cloud. This also further tempers the scorching heat by its
opacity, performing the acceptable office of a screen. From
thence, the clouds are carried to the inland countries, to form

the sources in the mountains, which are to supply the number-
less streams that water the fields.   And, by the steady opera-
tion of causes which are tolerably uniform, the greater part of
the vapours pass on to the circumpolar regions, there to de-.
scend in rains and dews ; and in this beneficent conversion
into rain by the cold of those regions, each particle of steam
gives up the 700 or 800 degrees of heat which were latent in
it.   These are immediately diffused, and soften the rigor of
those less comfortable climates.

Surely then, these two chemical laws of nature are curious,
of extensive influence, and of mighty importance.   The dis-
covery, and the satisfactory establishment of them, were titles
to fame and honor, and the name of Dr. Black should have
now been familiar among the philosophers of Europe.   About
this he gave himself little concern, and was perfectly satisfied
when he saw that his pupils understood the doctrine as de-
livered in the lectures.   One thing indeed gave him much
satisfaction.   Mr. Watt, whose worth of heart was now as
well known to Dr. Black as his excellent understanding, and
who was become his intimate friend, had obtained his Majes-
ty's patent for the improvement which he had made on the
steam-engine, by his judicious application of Dr. Black's in-
structions, and was now in the straight road to riches and
fame.   Dr. Black would scarcely have been more gratified,
had those advantages accrued to himself.   Their joint studies
had brought to light many unnoticed properties of vapours ;
and I believe that both the friends considered this period of
successful investigation as among the most fertile of enjoy-
ment of any part of their lives.   I had the pleasure of witness-
ing some of their inquiries, and sharing in the knowledge re-
sulting from them ; and to me also this period is matter of
the most pleasing recollection.

Meantime, Mr. Watt's engine became very generally known
through Europe.   Its immense superiority, in respect of power
and economy, offered to the busy part of society a most cer-
tain and powerful first mover for all machinery ; and thus
attracted the attention of all those engaged in the great busi-
ness of making money.   It was this, more than all the love of

knowledge, so boldly claimed by the eighteenth century, that spread the knowledge of the doctrine of latent heat, and the name of Dr. Black. Chemists, mechanicians, meteorologists, manufacturers, all took an interest in it ; and publications and plagiarisms multiplied apace. These were totally disregarded by the unambitious author, who, in the mean while, was happy in the success of his friend, and in the thoughts of having exerted his own talents so usefully for the public.

Dr. Black continued in the university of Glasgow from 1756 to 1766, much respected as an eminent professor, much employed as an able and most attentive physician, and much beloved as an amiable and accomplished gentleman, and happy in the enjoyment of a small but a select society of friends. Often, however, have I seen how oppressive his medical duties were to his spirits, when he saw that all his efforts did not alleviate the sufferings of the distressed. When his dear friend Dr. Dick was carried off, Dr. Black's distress was indeed exceedingly great, particularly, as he thought that another mode of treatment might have been more successful.

In the mean time, his reputation as a chemical philosopher was every day increasing; and pupils from foreign countries carried home with them the peculiar doctrines of his courses; and fixed air and latent heat began to be spoken of among the naturalists on the Continent. The progress however of this diffusion of knowledge must have been slow, had things continued in the same train. But in 1766 Dr. Cullen, chemical professor in Edinburgh, was appointed professor of medicine, and thus a vacancy was made in the chemical chair of that University. There was but one wish with respect to a successor. Indeed, when the vacancy happened in 1756, by the death of Dr. Plummer, the reputation of young Black was so high, as a person not only ingenious and inventive, but singularly correct and logical in his manner of thinking and writing, that, had the choice depended on the university, the newly graduated physician would have been professor of chemistry. He had now, in 1766, greatly added to his claim of merit, by his more important discovery of the procedure of nature in producing fluidity and vapor ; and he had acquired the high

esteem of all, by the singular moderation and scrupulous caution which marked all his researches.

Such a man was of the highest value to a celebrated seminary of learning. Ingenious men, of a fertile and lively imagination, are but too apt to give a loose to their fancy, in forming wide-grasping theories, and dressing them out in specious attire. The young student, ardent and credulous, is dazzled by what appears a strong and wide-spreading light, not remarking that perhaps it is not the natural emanation from a luminary, but is artificially collected by mirrors and glasses; or that what he takes for real objects are only the shadowy representations by a magic lanthorn. To this, in a great measure, may we ascribe the continual flux of theory which may be observed in all universities. Yet the consequences to science are most unfortunate. Not only do the precious years of youth and of mental energy pass on without solid instruction, but also the most unfortunate of all habits is acquired, that of considering the extensive and plausible application of a theory to the explanation of phenomena as a valid proof of its truth. But, on the other hand, the lectures of such a teacher as Dr. Black, never permitting this play of fancy, and even rarely introducing conjecture, would be safe lessons for ingenuous youth. The affirmations of the professor may be trusted as matter of experience, and the student will acquire betimes the habit of never proceeding, in research of any kind, without sounding the channel as he advances.

Dr. Black was appointed to the chemical chair at Edinburg, to the general satisfaction of the public; but the university of Glasgow thence sustained an irreparable loss. In this new scene, his talents were more conspicuous, and more extensively useful. He saw this; and while he could not but be highly gratified by the great concourse of pupils, which the reputation of the medical colleges of Edinburgh brought to his lectures, his mind was forcibly impressed by the importance of his duties as their teacher. This had an effect, of which it is difficult to say whether it has been fortunate for the public or not. Dr. Black now formed the firm resolution of directing his whole study to the improvement of his scholars

in the *elementary* knowledge of chemistry. He saw too many of them with a very scanty stock of previous learning. He had many from the workshop of the manufacturer, who had none at all; and he saw that the number of such hearers must increase with the increasing activity and prosperity of the country : and these appeared to him as by no means the least important part of his auditory. To engage the attention of such pupils, and to be perfectly understood by the most illite-rate, was therefore considered by Dr. Black as his most sa-cred duty. Plain doctrines, therefore, taught in the plainest manner, must employ his chief study. That no help may be wanting, all must be illustrated by suitable experiments, by the exhibitions of specimens, and the management of chemi-cal processes. Nice and abstruse philosophical opinions would not interest such hearers ; and *any* doctrines inculcated in a refined manner, and referring to elaborate disquisitions of others, would not be understood by the major part of an audience of young persons, as yet only beginning their studies.

To this resolution Dr. Black rigidly adhered, endeavor-ing every year to make his courses more plain and familiar, and illustrating them by a greater variety of examples in the way of experiment. No man could perform these more neatly and successfully. They were always ingeniously and judiciously contrived, clearly establishing the point in view, and never more than sufficed for this purpose. While he scorned the quackery of a showman, the simplicity, neatness, and elegance, with which they were performed, were truly admirable. Indeed, the *simplex munditiis* stamped every thing that he did. I think it was the unperceived operation of this impression that made Dr. Black's lectures such a treat to all his scholars. They were not only instructed, but (they knew not how) delighted ; and without any effort to please, but solely by the natural emanation of a gentle and elegant mind, cooperating, indeed, with a most perspicuous exhibi-tion of his sentiments, Dr. Black became a favorite lecturer; and many were induced, by the report of his students, to at-tend his courses, without having any particular relish for che-

mical knowledge, but merely in order to be pleased. This, however, contributed greatly to the extending the knowledge of chemistry; and it became a fashionable part of the accomplishment of a gentleman.

In the mean time, the path which had been opened by Dr. Black to a new province of chemical research began to be frequented by men of science in various parts of Europe. It was not only a most unexpected and curious thing to find that a matter so solid and impenetrable as marble could appear in the form of air, and this air be again put into our hands in the form of marble; but this new acquaintance had properties which forcibly called for the most serious attention. This air can be poured from one jar into another, like as much water; and when it is poured out on a candle, or even on a fire in sufficient quantity, they are extinguished in an instant, as if water had been poured on them. But further, should a man take one full inspiration of this air, this inspiration will be his last; he expires without a groan. In short, this is the deadly vapour which has often produced fatal effects in our mines. Being much heavier than common air, it glides downwards, collects in the drifts and lower parts of the mine, and sometimes is so copious as to fill them, and even to rise to a considerable height in the shaft. The unfortunate miner, let down by a rope to his work, as soon as his head gets under the surface of this fluid, drops off without any warning, and is dashed to pieces, or suffocated by this choke-damp. But we have now learned its property of extinguishing flame; and it is usual, before the workmen go down, to let down a choffer of burning coals, which are extinguished as soon as they enter the fixed air. I may add, that it is this fluid that has long drawn the curious traveller to the Grotto del Cane in Italy, so called because a dog falls down dead as soon as he reaches the middle of the grotto.

On the other hand, it has been discovered that this very air, so fatal, when applied to the nerves of the breathing organs, is most salutary, when received into the stomach; and, as an external application, it is most powerful in healing ulcerated wounds, cleansing foul sores, and in general, counteracting all

tendency to gangrene or putrefaction in the disorganised parts
of the animal frame.   Water impregnated with this air allevi-
ates one of the most excruciating of human sufferings, the
pain of the stone or gravel.

It has also been discovered that this air, so destructive and
so salutary, is forming in vast quantities every moment around
us.   Dr. Black discovered that the breathing of animals chan-
ges common air into fixed air ; and that this change is accom-
panied by the emersion of heat; which emersion seems to be
the principal source of the heat generated in the bodies of all
breathing animals. Another most copious source of fixed
air is the combustion of nine-tenths of all inflammable bodies.
Every thing, which, in the course of burning, suffers the change
which we call charring, changes common air into fixed air by
burning in it.   In this way does it sometimes happen that
persons have been found lifeless, who have been shut up in a
close room with a charcoal fire.   Lastly, another abundant
source of fixed air is the working of fermenting liquours, as
they are ripening into wine or other intoxicating beverages.
The froth and foam continually rising from such liquids is
one of the purest kinds of fixed air.

Surrounded, then, as we are by sources which are continu-
ally pouring in upon us this powerful substance, it behoves us
to be on our guard ; and we are highly obliged to him who
gave us the means of detecting, a method of removing it, and
methods for procuring it, when we would avail ourselves of its
salutary powers.   Somewhat of all this had been known be-
fore 1756.   The ingenious researches and experiments of Dr.
Hales had occasionally called our attention to some of the
modes of the production of fixed air ; but they appeared as sin-
gularities, insulated facts not connected in the general eco-
nomy of nature, or, at least, the connection was not observed.
But now, having learned a sure and easy test of the presence
of this kind of air, and understanding some of the means by
which nature holds it accumulated in such vast abundance,
and some of the methods for setting it at liberty, so as to ob-
tain it by itself, in, circumstances which lay it open to our ex-
amination, the chemists were enabled to detect it in almost

every body that could be presented to them, and were busy
in scrutinizing every substance with this view. They were
particularly curious to examine every elastic eruption that
they observed, on account of its resemblance to those erup-
tions in which fixed air is extricated. The effervescence of
metals during their solution in acids resembles so much the
effervescence of acids and alkalis,....the frothy ebullition of
some putrescent mixtures resembles so much the fermentation
of wines and worts, that curiosty led immediatdly to the ex-
amination of the elastic matter which was extricated on those
occasions. These airs, however, were found altogether dif-
ferent from fixed air; but this circumstance only fired the cu-
riosity of the inquirers so much the more, and incited them to
multiply experiments, and examine every body, in order to
extricate or to create some elastic matter for a new subject of
experiment.

Thus arose a new species of chemistry, chiefly conversant
with aërial fluids, having an apparatus and manner of proceed-
ing altogether peculiar to itself, and so unlike all that we are hi-
therto acquainted with, that every thing may be said to be big
with curiosity and with novelty. This department of chemi-
cal science got the name of PNEUMATIC CHEMISTRY.

Of all those who were occupied in these researches, the
most eminent were Dr. Priestley and Dr. Scheele. Dr. Priest-
ley, by the number and variety of his experiments, and the
substances he discovered ; and Dr. Scheele, by the ingenuity,
and the unwearied patience with which he examined the in-
dividual novelty which engaged his attention, and the saga-
city with which he contrived his experiments, so as to lead
him with certainty to some important result, to be added to
our former stock of chemical knowledge. These two philo-
sophers, unknown to each other, discovered the same sub-
stances ; substances which were acting the most important
parts in the great operations of nature. Both of them discov-
ered that aëreal fluid which alone sustains the life of breath-
ing animals, and which alone supports inflammation and com-
bustion ; for which reasons it has been called *vital air* and *em-
pyreal air*. Both of them also discovered another species of

air, much more abundant, and indeed proved by Scheele to form the chief portion of our atmosphere. He called it *foul* or *putrid air*, having discovered it first in the putrescent fermentations. Dr. Priestley procured it from much less offensive materials, and called it *phlogisticated air*. It extinguishes life and fire as certainly as the fixed air of Dr. Black does, but is quite a different substance. Scheele first demonstrated that our atmosphere is a mixture, but not a compound, of this and of vital air. Dr. Priestley also discovered some of the means by which nature removes all the taints which are occasioned in the atmosphere by the breathing of animals, the burning of fuels, and the fermentation of bodies, shewing that these corruptions are combinations of vital air with other elastic matters, and that all of them are again decompounded in the process of vegetation, so that the plants restore to us the vital air in its original purity; thus accomplishing one of the grand and beneficent circles of natural operations.

Such discoveries necessarily gave a dignity to pneumatic chemistry, which sets it very high in the rank of natural sciences. It is no longer confined to the study of those properties of bodies which make them the subjects of human art, by which they are worked up for our purposes. We are now admitted into the laboratory of nature herself, and instructed in some of those great processes by which the author of this fair world makes it a habitable place, and a never-failing source of life and enjoyment, by a circle of beneficent changes, in which the same materials are made the means of enjoyment to successive races of inhabitants, and are again and again presented in their original purity and usefulness.

So captivating to every mind of sensibility, it was no wonder that pneumatic chemistry became a very general study, and engaged the eager attention of the most accomplished in acquirements, and the most eminent in the ranks of society. In Germany, in Italy, in Britain, in France, it found cultivators in every class of society. The aëreal fluids having now become as familiar, and as easily managed, as the tangible substances, which we are accustomed to hammer, to grind, to dissolve, and distil, they were mixed and subjected to all the

torturing degrees of heat, and passed from retort to retort,
and, in short, were examined in every way that imagination
could suggest. Such a pertinacious scrutiny of nature could
not fail of bringing many things to light. The honorable
Mr. Cavendish has discovered that aquafortis consists of two
kinds of air; of vital air, the support of life and of fire, and
mephitic air,* which extinguishes both. And he discovered
that water, which, since the first dawn of natural philosophy,
has been considered as an element, is also composed of vital
air, and of that air which sometimes takes fire in our coal pits,†
and lays all waste by its explosion. Others have discovered
that salt of hartshorn consits of inflammable and mephitic air;
nay, that fixed air itself consists of diamond dissolved in vi-
tal air.

In the midst of this ardor of research, and this rich harvest
of discovery, Mr. Lavoisier appeared, and took an active
share;....not hunting after new substances, he considered
those already known, with more sagacity than the multitude
busy in the chace. He thought that the chemical relations of
various substances had been mistaken by all; that we hold
many bodies as simple, of which we can shew the composi-
tion, and those to be compounded which are really more sim-
ple. Thus, sulphur, which the chemists, ever since the days
of Stahl, have supposed to consist of vitriolic acid, and that
matter which imparts inflammability to bodies, was proved
by Lavoisier to be more simple than the acid, and that this
acid was in fact composed of sulphur and vital air. He
proved that in the phenomenon which we call combustion
or inflammation, the only thing of which we are absolutely
certain is the combination of the inflammable body with vital
air; and that, by separating this air from it again, the body
regains its primitive form, and is again inflammable, that is,
again capable of uniting, in a particular way, with vital air.
Reflecting now on the two cases in which Dr. Black had dis-
covered a combination of tangible matter with fire, in such a
way as not to be discovered by the temperature, but only by
the liquid or the vaporous form which it causes the substance

* Or azotic gas.   † Hydrogenous gas.

to assume, he asserted the reality of a third combination of
tangible matter with heat, to be added to those discovered by
Dr. Black : a combination, which was not to be changed by
the mere contact of a sufficient quantity of *any* cold matter,
but required the contact of another substance, properly related
to vital air in the way of chemical affinity. Heat combined
in this manner renders a fluid *a real*, or permanently elastic,
and no longer condensible like watery vapors. These com-
pounds he denominated *gases*. Of this kind are all the airs
lately discovered. Lastly, as the chief point of his doctrines,
he affirms that the light and heat which appear in the com-
bustion, are ingredients of the vital air, detached from it, and
from it alone, when its ponderable part combines with the
body that we call inflammable.

The doctrine is not altogether new. Dr. Robert Hooke,
one of the first members, and the brightest ornaments of the
Royal Society of London, published the same doctrine in his
Micrography, 1665 ; and Dr. Mayhow of Oxford entertained
opinions extremely similar ;....Rey, a French chemist of that
time, had similar conjectures. But by some unaccountable
fatality, these publications were forgotten.

This doctrine concerning combustion, the great, the cha-
racteristic phenomenon of chemical nature, has at last receiv-
ed almost universal adoption, though not till after considera-
ble hesitation and opposition; and it has made a complete re-
volution in chemical science.

But it is thought that the cultivation of pneumatic chemistry
has given us even more important information ; and it promises
acquisitions of knowledge of a still more elevated rank ; it pro-
mises admission to the more mysterious operations of nature
in the functions of vegetable and animal life. For, by the pneu-
matic analysis, to which organized bodies have been subjected
by some of the eminent chemists of the present day, it appears
that they consist of a very small number of simple substances,
capable of the a real form, but existing in the plant or animal
in a liquid or a concrete state, in consequence of being differ-
ently combined or associated. The laws of many of those
affinities have been discovered ; and it is thought that we per-

ceive, in many cases, how, by a change of temperature, or by presenting another ingredient, these combinations change, new forms are assumed, and the distinguishing products of animal and vegetable bodies appear. It is thought that we perceive, in some cases, how the functions of the organized body produce this variation of temperature, or this change of situation among the ingredients, which occasions the chemical combination observed.

On the other hand, we have now got some very distinct indications of the internal procedure of nature in those spontaneous fermentations which take place in the contents of animal and vegetable bodies independent of their vital functions and which at last destroy them, or reduce them to brute unorganized matter. These changes are thought to proceed from small variations of temperature or position, which, by changing the mutual forces of attraction, destroy that equilibrium of force which maintains things in their present condition. The particles change their partners, (so to express myself) and in their new combinations, present to our view substances which were not existing before in the animal or vegetable matter. Thus does the sweet juice of the grape give us wine, which did not exist before....and this gives us inflammable spirits, which did not exist in the wine, or gives us sour, mawkish, or fœtid liquors or fumes, none of which had any previous existence.

Justly, therefore, have I said that chemistry has risen to a high and unexpected rank in the scale of science, important to society, by the vast additions which it has made to the power of man, and precious to the philosopher, by the connections which it has brought into view between the different agents in the grand circle of natural operations which constitute growth, life, decay, and final destruction. I think myself equally entitled to say, that it was the two discoveries of Dr. Black....fixed air, and combined heat, which gave the incitement, pointed out the road, and furnished the chief helps for pursuing it.

It is reasonable to suppose that he took an active part in those keen researches, which have thus occupied the attention of the philosophers. But alas!...."his lot forbade." His

constitution had always been exceedingly delicate. The
slightest cold, the most trifling approach to repletion, imme-
diately affected his breast, occasioned feverishness, and, if con-
tinued for two or three days, brought on a spitting of blood.
In this situation, nothing restored him to ease but relaxation
of thought and gentle exercise. The sedentary life to which
study confined him was manifestly hurtful ; and he never al-
lowed himself to indulge in any intense thinking, or puzzling
research, without finding these complaints sensibly increased.

Thus situated, Dr. Black was obliged to be contented as
the spectator of the successful labours of others. So com-
pletely trammelled was he in this respect, that although his
friends saw others disingenuous enough to avail themselves of
the novelties announced by Dr. Black in his lectures, without
acknowledging the obligation, and were thence afraid that
their friend's claim of originality and priority might become
doubtful ; and although they repeatedly urged him to publish
an account of what he had done, this remained unaccomplish-
ed to the last. Dr. Black often began the task ; but was so
nice in his notions of the manner in which it should be exe-
cuted, that the pains he took in forming a plan of the work
never failed to affect his health, and oblige him to desist. Of
this I saw a most distinct instance, when his dissatisfaction
with the artful conduct of Mr. Lavoisier provoked him to
make an unusual exertion.*

* I embrace this earliest opportunity that occurs to correct a mistake which
I have made in page   of Vol. II. where I say, " Nor is he named in those
" passages of the earlier dissertations where the character and properties of
" fixed air, and of mild and caustic alkalis, are treated of." The words
" *of the earlier dissertations*," should not have been there. The observation
relates only to the joint memoirs of Lavoisier and La Place, published by
the Royal Academy of Sciences. Mr. Lavoisier, in his first publication of
Opuscles Physiques et Chymiques, in 1774, gives a brief account of all that
had been published or taught concerning quicklime, and fixed air, and effer-
vescence. And Dr. Black's performance has its share of his attention, and
his final approbation. But I must say, that it is with none of those expres-
sions of esteem and respect which Mr. Lavoisier professes to have always
entertained for the author. Dr. Black, at the time that he was offended
with Mr. Lavoisier's insincerity, knew perfectly that he had taken notice of
his doctrines concerning quicklime, &c. in that early dissertation : but
the ground of offence was recent, and gross.

Dr. Black, therefore, devoted his whole time and attention to the communications which his pupils had a right to expect from him. Moderate in all his wishes, he was never anxious to bring himself into view, unless the occasion required his appearance. His reputation naturally engaged him in an extensive correspondence, he being often appealed to as a judge, and often consulted as a philosopher. On such occasions, when he could give his opinion without being obstrusive, (a thing which he detested) he was ever ready to communicate it, and to give every useful information....which he did with frankness and sincerity, and with the most unaffected modesty.

As to the manner in which Dr. Black acquitted himself in his public character of a professor, I need only say that none contributed more largely to establish, and support, and increase, the high character which the University of Edinburgh has acquired. His talent for communicating knowledge was not less eminent than for observation and inference from what he saw. He soon became one of the principal ornaments of the University; and his lectures were attended by an audience which continued increasing from year to year, for more than thirty years. It could not be otherwise. His personal appearance and manner were those of a gentleman, and peculiarly pleasing. His voice in lecturing was low, but fine; and his articulation so distinct that he was perfectly well heard by an audience consisting of several hundreds. His discourse was so plain, and perspicuous, his illustration by experiment so apposite, that his sentiments on any subject never could be mistaken, even by the most illiterate; and his instructions were so clear of all hypothesis or conjecture, that the hearer rested on his conclusions with a confidence scarcely exceeded in matters of his own experience.

I have already observed, that the strong sense which Dr. Black entertained of the duties of his professional situation, precluded all extensive medical practise, which otherwise he had every talent, and every accomplishment fitted to insure. He restricted his attendance as a physician to a few families of intimate and respected friends. He was, however, " a

" physician of good repute, in a place where the character
" of a physician implies no common degree of liberality, pro-
" priety and dignity of manners, as well as of learning and
" skill."

Averse, by disposition, from ostenation, or any inclina-
tion to obtrude his opinions, on the public, Dr. Black peculiarly
disliked appearing as an author. His dissertation, *De Acido
a cibis orto, et de Magnesiâ*, was a work of duty, being his In-
augural Thesis. His *Experiments on Magnesia, Quicklime,
and other alkaline substances*, printed soon after, was almost
indispensably necessary for putting on a proper foundation
what was only indicated in his inaugural dissertation. His
*Observations on the more ready freezing of water that has been
boiled*, published in the Philosophical Transactions of Lon-
don in 1774, was also called for; and his *Analysis of the
waters of some boiling springs in Iceland*, made at the request
of his friend T. J. Stanley, Esq. was read to the Royal Socie-
ty of Edinburgh, and published by the Council. Dr. Black
was perhaps fastidiously nice in his notions of a philosophical
performance, and too severe in his observations on the hurri-
ed publications of some chemists, which he used to call slo-
venly, and to consider as literary manufacture for profit. But
surely, every man who gives the public a new and important
fact, confers a public benefit; and when he adds his own re-
flections and opinions, he only shews what have been his own
incitements to exertion. Few persons are so insensible to or-
dinary propriety, as to make pretensions to authority; and if
they did, it would be disregarded, while the philosopher would
avail himself of the information communicated. Had Dr.
Priestly and Dr. Scheele been as fastidious as Dr. Black, we
might at this day have been still ignorant of the chief doc-
trines of chemical philosophy. But such was Dr. Black's
aversion to all hypothesis and conjecture in any experimental
science, that he could not endure the title of a *system* to be
given to any body of chemical doctrines yet published; and
he did not call his own discourses *Lectures on Chemistry*, but
Lectures on the Effects of Heat and Mixture; so far did he
think all his endeavours were from forming a system of the

chemical department of science. In the last years of his life, he
was convinced of the propriety of this scrupulousness, by the
precipitancy with which he saw young men, who had scarcely
left the forms of the school, publishing in all quarters of Eu-
rope.   Intoxicated, as it were, with the large draughts of in-
formation afforded by pneumatic chemistry, they think them-
selves adequate to the task of giving a system of this almost
boundless science,....a system which shall not leave a phe-
nomenon unexplained; and they obtrude these their crude
conceptions on the public with most unbecoming confidence
and authority.   He saw the public pleased with this manner
of proceeding, and far from being scrupulous about the solidi-
ty of the foundation, provided the structure be shapely and
extensive.   He dreaded the consequences of this passion for
theories.   He therefore resolved to abide scrupulously by
his first plan, which he had adopted in the hour of calm re-
flection, and had modelled as much as he could on the rules
of philosophising, so warmly inculcated, and so scrupulously
followed by the illustrious Newton. Scheele, Bergmann, and
Berthollet, were the chemists of later times whom he thought
most highly of, as Margraaf and Crammer were most admired
by him among those of older date.   He corresponded occasi-
onally with Seguin, and with Crell, who had been his pupil; but
did not encourage much intercourse of this kind, having found
that his informations sometimes appeared in print as the in-
vestigations of the publishers.   He could not be engaged to
transmit any essay to the Royal Academy of Sciences at Pa-
ris, or the Imperial Academy at St. Petersburgh, of both of
which he had been elected a foreign associate.

Such was Dr. Black, considered as a public man I wish
that I could as easily describe him in his private capacity,....at
home, or in society,....as an acquaintance, or a friend : but
this requires a talent to which I have no pretensions.   It is
not a very difficult matter to draw a figure, which shall shew
with abundant accuracy, any peculiarity of a man's dress, and
perhaps even give somewhat of his air; but the delicate strokes
which mark his features, and shall make us know the man, re-
quire the hand of a painter.

I have already observed, that when I was first acquainted with Dr. Black, his aspect was comely and interesting. As he advanced in years, his countenance continued to preserve that pleasing expression of inward satisfaction, which, by giving ease to the beholder, never fails to please. His manner was perfectly easy, and unaffected, and graceful. He was of most easy approach, affable and readily entered into conversation, whether serious or trivial. His mind being abundantly furnished with matter, his conversation was at all times pertinent and agreeable: for Dr. Black's acquirements were not merely those of a man of science. He was a stranger to none of the elegant accomplishments of life. He therefore easily fell into any topic of conversation, and supported his part in it respectably. He had a fine or accurate musical ear, and a voice which would obey it in the most perfect manner; for he sung, and performed on the flute, with great taste and feeling; and could sing a plain air at sight, which many instrumental performers cannot do. But this was science. Dr. Black was a very intelligent judge of musical composition; and I never heard any person express so intelligibly the characteristic differences of some of the national musics of Europe. I speak of Dr. Black as I knew him at Glasgow: after his coming to Edinburgh, he gave up most of those amusements. Without having studied drawing, he had acquired a considerable power of expression with his pencil, both in figures and and in landscape. He was peculiarly happy in expressing the passions; and seemed, in this respect, to have the talent of a history painter. He had not had any opportunities of becoming a connoisseur; but his opinion of a piece of painting, or sculpture, was respected by good judges. Figure, indeed, of every kind, attracted his attention;....in architecture, furniture, ornament of every sort, it was never a matter of indifference. Even a retort, or a crucible, was to his eye an example of beauty or deformity. His memorandum books are full of studies (may I call them) of this sort; and there is one drawing of an iron-furnace, fitted up with rough unhewn timber, that is finished with great beauty, and would not disgrace the hand of a Woollet. Naturally, therefore, the young ladies

were proud of Dr. Black's approbation of their taste in mat-
ters of ornament.   These are not indifferent things ; they are
features of an elegant mind, and they account for some part
of that satisfaction and pleasure which persons of all different
habits and pursuits felt in Dr. Black's company and conver-
sation.

I think that I could frequently discover what was the cir-
cumstance of form, &c. in which Dr. Black perceived or
sought for beauty,....it was some suitableness or propriety ;
and he has often pointed it out to me, in things where I never
should have looked for it.   Yet I saw that he was ingenious-
ly in the right.   I may almost say that the love of propriety
was the leading sentiment of Dr. Black's mind.   This was
the first standard to which he appealed in all his judgements ;
and I believe he endeavoured to make it the directing prin-
ciple of his conduct.   Happy is the man whose moderation
of pursuits leaves this sentiment in possession of much au-
thority.   Seldom are our judgments greatly wrong on this
question ; but we too seldom listen to them.

Dr. Black had the strongest claim to the appellation of a
man of propriety and correctness.   His friend Dr. Fergu-
son knew him well, and can delineate his moral features in-
finitely better than I can.   Dr. Ferguson says of him,....

" As Dr. Black had never any thing for ostentation, he
" was, at all times, precisely what the occasion required, and
" no more.   Much as he was engaged in the details of his
" public station, and chemical exhibitions, his chambers were
" never seen lumbered with books and papers, or specimens
" of mineralogy, &c. or the apparatus of experiments. Nor did
" any one see Dr. Black hurried at one time to recover mat-
" ter which had been improperly neglected on a former occa-
" sion.   Every thing being done in its proper season and
" place, he ever seemed to have leisure in store ; and he was
" ready to receive his friend or acquaintance, and to take his
" part with cheerfulness in any conversation that occurred.
" And, let me remark, that no one ever with more ease to
" himself refrained from professional discussions of any sort,
" or conversation in which he was acknowledged superior....

" or with less self-denial, in mixed company, left the subject
" of conversation to be chosen by others." Yet was he far
from maintaining a silence indicating either indifference or
neglect; on the contrary, he loved to promote social conver-
sation by every cheerful thought that occurred.   " Many
" years member of a society of noblemen and gentlemen of
" the first rank, of judges, lawyers, military men, and pro-
" fessed men of letters, he kept his place with the most easy
" propriety, having knowledge sufficient for giving him an
" interest in the conversation of each, and for taking a res-
" pectable share in it, without exhibiting any peculiarities
" arising from his more accustomed habits of thought." This
society, and another small evening party, or club of gentle-
men, more professedly scholars, were the only public compa-
nies which his delicate health permitted him to frequent.

But Dr. Black had neither the temper nor the feelings of a
recluse.   He loved society, and felt himself beloved in it.   In
coming to Edinburgh, he had the happiness of rejoining his
friend, Mr. Adam Smith, whose society and friendship had
given him so much delight in Glasgow.   The same promi-
nent feature of character, " perfect singleness of heart," void
of all guile, attached him warmly to Mr. David Hume ; and
the attachment was mutual and equally confidential.   His
relation Dr. Adam Ferguson, Mr. John Home, Dr. Alexan-
der Carlyle, and one or two other gentlemen of talents and of
elegant accomplishments, were his chief society in his hours
of relaxation.   His professional eminence was not the bond
of this acquaintance ; for their attachment to him, and to each
other, arose from their experience of ingenuity, and candor,
and good taste, rather than from any similarity of studies, or
agreement of opinions.

But there were others of Dr. Black's intimate friends, to
whom his philosophical talents, and more particularly his
chemical and geological knowledge, were powerful sources of
attachment.   Mr. Clerk of Elden, and his brother Sir George,
Dr. Roebuck, and Dr. James Hutton, were ever as ready to
receive information from him as he could be to impart it ;
and in their society he could indulge in his professional

studies. To the last of these gentlemen Dr. Black was most affectionately attached; and in respect to habits of intimacy, Dr. Hutton should perhaps have been placed first on this list. " He made up in physical speculation all that was wanting in " any of the rest of his acquaintance. Yet would it be diffi- " cult to say whether the characters of Dr. Black and Dr. " Hutton, so often seen together, were most to be remarked " for resemblance or contrast. Both profound in physical " science; both rigid adherents to fact, in exclusion of all " hypothesis, or the most specious conjecture: both of con- " summate humanity and candor. Dr. Black was serious, " but not morose. Dr. Hutton playful, without petulance. " The one was always on solid ground; and of him it might " be said, *nil molitur ineptè*. The other, whether for plea- " santry, or serious reflection, could be in the air, speculate " beyond the laws of nature, or their phenomina, and treat " the common notion of body, extended and impenetrable, as " a vulgar error." But, with all this diversity of relish, the friends were united by mutual respect for the talents of each other, and the most implicit confidence in each other's inte- grity and worth. Dr. Hutton was the only person now near him, to whom Dr. Black imparted every speculation in che- mical science, and who knew all his literary labours. Seldom were the friends asunder for two days together. Mr. Watt was now at a distance, in Birmingham; but they kept up a close and philosophical correspondence, in which all their speculations and projects were known to each other. Soon after his coming to Edinburgh, the Doctor got another pupil, Mr. Archibald Geddes, manager of the glass-works at Leith, who soon engaged his Professor's attention by the readiness and propriety with which he applied to the improvement of his manufacture the instructions which he received in the lec- ture. Farther acquaintance shewed more to esteem and at- tach; and it terminated in the most intimate and confidential friendship. From this friend no circumstance of Dr. Black's former life or his present condition was withheld; and to his assistance he had recourse in every thing that affected either his fortune or his comfort.

In this society Dr. Black passed his days calmly, but cheerfully, respected and beloved, and conscious that he was worthy of this regard. Though unmarried, his house was not unoccupied; for the uniformity of his single life was often agreeably chequered by the welcome visits of his numerous friends, the descendants of the respectable pair at Bordeaux. In these domestic scenes he appeared to the best advantage, for he enjoyed them most. Large companies were not to his taste. Parties of men of learning, he thought, had seldom much of conviviality; this requires conversation of more general interest, and where the affections are rather engaged than the understanding.

My narration now draws towards a close. The infirmities of advanced life now bearing more heavily on a feeble constitution, gradually curtailed those hours of walking and gentle exercise which had always been necessary for Dr. Black's ease. Company and conversation began to fatigue; he went less abroad, and was visited only by his intimate friends. His duty at College now became too heavy a task, and he got an assistant, who took a share of the lectures, and relieved him from the fatigue of the experiments. But, at length, even this was more than enough for his diminished strength, and he was obliged to give over lecturing altogether. But all this gradual decline, and occasional short periods of more than common indisposition, made no change in the gentle cheerfulness of his disposition and manners. A friend was always received with unaffected welcome; and whenever he found himself engaging more in conversation than was consistent with his health, he said so; and said it in a manner so pleasing, that his guest was only the more induced to repeat his visit.

I cannot conclude what should be said of this amiable person so well as in the words of Dr. Ferguson, who says,

" The life of Dr. Black was not less distinguished by cor-
" rectness and propriety of conduct, than by ingenious rea-
" soning and scientific research. For he carried into his
" private affairs the same order and good conduct that he em-
" ployed in his philosophical studies, and in his professional

" duties.   And he reaped through life the benefit of his at-
" tention to this particular, in the ease of his circumstances,
" and in the power which it gave him on occasion to assist his
" friends, or to contribute to the promoting of any public con-
" venience.

   " From those, indeed, with whom mere remissness or ne-
" glect is allowed to pass for generosity, Dr. Black may have
" been thought too attentive to the increase of his fortune.
" But they did not know him*; and if they were required to
" substantiate the charge, the proofs which they would ad-
" duce, would be found indistinguishable from the real effects
" of sound reason and good sense.   His expences were in-
" deed regulated, but in no way unbecoming his station.   His
" house was spacious; and his table, at which he never im-
" properly declined any company, was plentiful and elegant,
" ....rather above than below his condition.   His contribu-
" tions for all public purposes were liberal, and like a gentle-
" man; and his purse was open to assist his friend.†   Much
" of his practice as a physician arose from his previous con-
" nection with the patient as a friend; and he was as assidu-
" ous where he would not accept, or could not expect a fee,
" as in the most lucrative part of his profession."

   I have already said that Dr. Black's constitution never was
robust, and that, as he advanced in years, it became gradually
more delicate and frail; so that every cold he caught occa-
sioned some degree of spitting of blood.   Yet he seemed to
have this unfortunate disposition of body almost under com-
mand, so that he never allowed it to proceed far, or to occa-
sion any distressing illness; and he thus spun his thread of
life to the last fibre; and even this does not seem to have
broken, but merely to have ended.   " He guarded against
" illness, by restricting himself to a moderate, or I should

   * About the time that he left Glasgow, he lost three-fourths of all the
fruits of his labours, by the failure of the house where he had lodged his
money.   He foresaw this failure for two years; yet no man ever observed
the smallest appearance of fretfulness, or any alteration of his behaviour to
the person by whom he was to suffer so severely.

   † I could give more than one or two instances in which a great part of his
fortune was at risk for his friend.

" rather call it, an abstemious diet ; and he met his increas-
" ing infirmities with a proportional increase of attention and
" care,....regulating his food and exercise by the measure of
" his strength.   It is wonderful with what skill and success
" he thus made the most of a feeble constitution, by thus pre-
" venting the access of disease from abroad.   He enjoyed a
" health which was feeble indeed, but scarcely interrupted,
" and a mind ever undisturbed, in the calm and cheerful use
" of all his faculties.   A life so prolonged had the advantage
" of present ease, and the prospect, when the just period
" should arrive, of a calm dissolution."....His only apprehen-
sion was that of a long continued sick-bed ; and this perhaps
less from any selfish feeling, than from the humane conside-
ration of the trouble and distress occasioned to attending
friends ; and never was this modest and generous wish more
completely gratified.   " On the 26th Nov. 1799, and in the
" seventy-first year of his age, he expired, without any con-
" vulsion, shock, or stupor, to announce or retard the ap-
" proach of death.   Being at table, with his usual fare, some
" bread, a few prunes, and a measured quantity of milk, di-
" luted with water, and having the cup in his hand when the
" last stroke of his pulse was to be given, he had set it down
" on his knees, which were joined together, and kept it steady
" with his hand, in the manner of a person perfectly at ease ;
" and in this attitude expired, without spilling a drop, and
" without a writhe in his countenance ; as if an experiment
" had been required to shew to his friends the facility with
" which he departed."   His servant opened the door to tell
him that some one had left his name, but getting no answer,
stepped about half way towards him, and seeing him sitting
in that easy posture, supporting his bason of milk with one
hand, he thought that he had dropped asleep, which he had
sometimes seen happen after his meals.   He went back, and
shut the door ; but before he got down stairs, some anxiety,
which he could not account for, made him return and look
again at his master.   Even then, he was satisfied, after coming
pretty near him, and turned to go away ; but again returned,
and coming quite close to him, he found him without life.

" So ended a life, which had passed in the most correct ap-
" plication of reason and good sense to all the objects of pur-
" suit which providence had prescribed to his lot ;" with
many topics of agreeable recollection, and few things to ruffle
his thoughts. He had long enjoyed the tender and affection-
ate regard of parents whom he loved, honored, and revered ;
with the delightful consciousness of being a dutiful son, and
being cherished as such ;....one of a family remarkable for
sweetness of disposition and manners, he had lived with his
brothers and sisters in terms of mutual love and attachment.
He had never lost a friend, but by the stroke of mortality,
and he felt himself worthy of that constancy and regard. He
had followed a profession altogether to his taste ; and had
followed it in a manner, and with a success, which procured
him the esteem and respect of all competent judges, and set
his name among the most eminent, and he was conscious that
his reputation was not unmerited ; and with a success, in res-
pect of emolument, which secured the respect, even of the
ignorant; which gave him the command of every rational
gratification, and enabled him to add greatly to the comforts
of the numerous descendents of his worthy parents,....heirs,
not only of their name, but likewise of their unambitious mo-
deration, and amiable simplicity of character. " The fortune
" that he left shewed how much he had profited by the order
" and just arrangement which he had ever maintaned in his
" affairs, amounting almost to the double of what any one
" thought that his income and his frugality could have amass-
" ed. The whole was disposed of by will, in a most accu-
" rate and satisfactory manner, parcelled into shares, accord-
" ing to the degree in which each individual was 'the object
" of his care and solicitude."

> His saltem accumulem donis, et fungar inani
> Munere.............
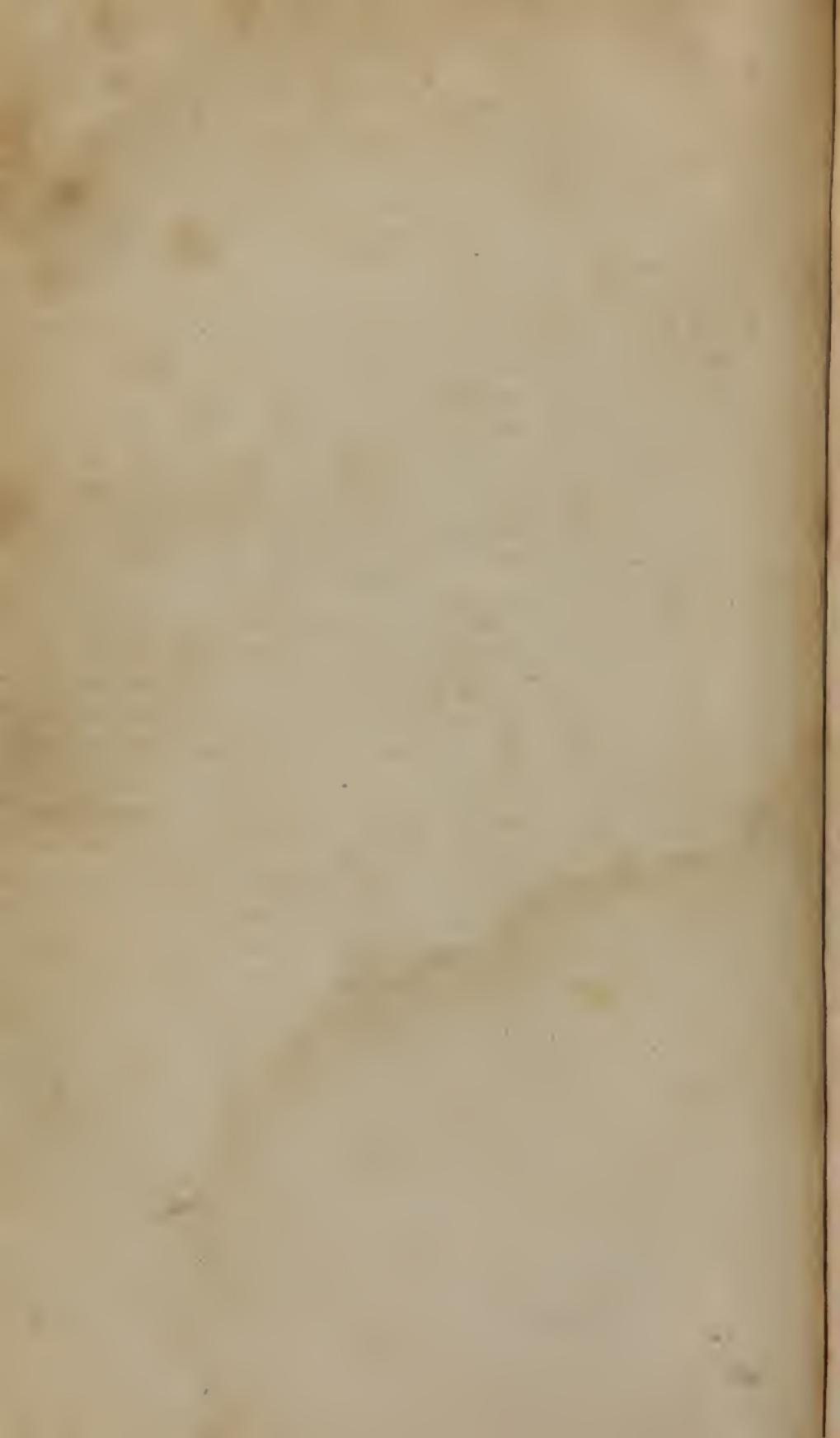
LECTURES

ON

CHEMISTRY.

OF CHEMISTRY IN GENERAL.

CHEMISTRY, like all other sciences, has arisen from the reflections of ingenious men on the general facts which occur in the practice of the various arts of common life. It will not greatly conduce, gentlemen, to your progress in the science, to trace chemistry from its first humble appearance in the practice of the more ingenious artists and manufacturers, till, by a copious collection and judicious arrangement and comparison of those practices, men still more ingenious and speculative deduced certain fixed laws of material nature, according to which all the chemical phenomena proceeded, and thus have raised this department of study to the rank and character of a science, or system of general doctrines, expressive of those laws of nature, and so arranged, according to the principles of sound logic, as to carry evidence and conviction into its various branches, and to furnish maxims and precepts for successfully directing the practice in a great variety of the useful arts. This, though not unusual in the beginning of such a course of lectures, cannot, I imagine, be attended with any considerble advantage, because, to persons altogether unacquainted with the chemical facts, and even with the substance in the hands of the chemist, such a narration would be little more than the mentioning of

many names, and telling you something of each, of which you could not form any distinct conception.

I hold it better, therefore, to turn your whole attention to the present state of chemistry, and give you such a description of it as shall correspond with our present attainments in the science, fully comprehending them all, while it shall exclude every thing foreign from our proper study. I shall give you a description or definition of it. This being somewhat peculiar, I must beg your indulgence, while I give my reasons for being dissatisfied with the definitions which have been given of chemistry by others; and I trust that you will not think your time misemployed in attending to them, because, in stating my objections to the definitions given by others, I shall be led to point out the essential differences between chemistry and other branches of knowledge with which it has been confounded.

In the first place then, one manifest impropriety in the greater part of the definitions of chemistry is, that their authors represent chemistry as an art. This error is to be seen in the definitions given by authors of the highest name, for such surely were Boerhaave and Stahl. Yet it is plain, from the writings of those authors, that their idea of chemistry did not correspond with such a denomination. They were probably led into this mistake by confining their attention too much to the chemical books which appeared before the beginning of this century. The greater number of those books had, indeed, for their principal object, a chemical art....the art of pharmacy; and, although they bore the title of courses of chemistry, they contained little more than the detail of processes, or rules, whereby a great number and variety of substances were prepared for the uses of medicine, with occasional services to other arts, particularly metallurgy. At the same time, also, those commonly known by the name of chemists were chiefly, or solely, artists, employed in making certain chemical preparations and products, according to rules and directions which they had learned to observe. But surely such persons, confining themselves to the exercise of an art, and, perhaps, describing and teaching this art in the most judicious and

complete manner, are still upon a footing with all other che-
mical artists, such as brewers, distillers, dyers, and many
others ; and, if we choose to apply the term chemist to them,
we must find some other term to distinguish from these ar-
tists such men as Sir Isaac Newton, Mr. Boyle, Cavendish,
Priestley, Boerhaave, Scheele, Bergmann, Lavoiser, and
others, who have improved our science. The public now per
ceive the distinction, and in some measure make it, by calling
the chemical artists TRADING CHEMISTS.

From what I have now said, you may perceive something
of the distinction which I think necessary to keep in view be-
tween art and science, between the artist and the man of
knowledge, or the philosopher. The man of knowledge, the
philosopher, is he who studies and acquires knowledge in or-
der to improve his own mind ; and with a desire of extending
the department of knowledge to which he turns his attention,
or to render it useful to the world, by discoveries, or by in-
ventions, which may be the foundation of new arts, or of im-
provements in those already established. Excited by one or
more of these motives, the philosopher employs himself in
acquiring knowledge and in communicating it. The artist
only executes and practises what the philosopher or man of
invention has discovered or contrived, while the business of
the trader is to retail the productions of the artist, exchange
some of them for others, and transport them to distant places
for that purpose.

I may illustrate this distinction between the man of science
and the artist still more by an example or two. While Sir
Isaac Newton was employed in his experimental inquiry con-
cerning the nature and laws of light, and was led by his ge-
nius from one beautiful discovery to another, until he pro-
duced the admirable work which he has left us on this sub-
ject, he was acting the part of the philosopher. His obser-
vations and discoveries induced him to believe that, by reason
of the nature of light, and the manner in which it is affected
by transparent refracting mediums, the telescopes in use were
capable of only a very limited degree of magnifying power.
But the manner in which it was reflected exhibited no such

obstacles to amplification.   He therefore proposed that teles-
copes should be attempted, which should perform their effect
by reflection; by which he saw that a much more perfect and
distinct image might be formed.   He went still farther: he
tried different mixtures of metals, to learn which would form
the best composition for the mirrors which must be employed
in these telescopes instead of the glass lenses hitherto em-
ployed.   He even tried different ways of grinding and polish-
ing those mirrors, in order to learn how to give them the
most perfect figure and the most exquisite polish, so as to
reflect the brightest and most perfect image.   After having
thus completed all the improvements he had projected, al-
though he published directions for making a reflecting teles-
cope in perfection, he was, in all this, still acting the part of
the philosopher.   The person who merely put in practice the
directions given by Newton, following implicitly the rules
which he had laid down, was the humble artist.   Sir Isaac
Newton, for want of good workmen, acted this part also, in
all its detail, being unable to obtain a good reflecting telescope
in any other way than that of making it himself, according
to his own rules.

In like manner, we find numerous operators who, either
with their own hands, or by the hands of others whom they
employ, exercise the various branches of the valuable art of
pottery.   These persons, by an apprenticeship, or otherwise,
have learned how to choose and to mix the proper materials;
how to form the vessels; to apply the glazing and other de-
corations; and lastly, how to give the proper degree of fire
to consolidate and finish the ware.   These are all artists,
while they only exert in practice the skill they have acquired,
whether by communication from others, or by efforts of their
own ingenuity.   But if there be a Wedgewood among them,
who takes pleasure in attaining more extensive knowledge of
the subject, who, by comparing the practice of other potters
with his own, and by making new trials, and varying the com-
position, the glazing, the firing, and other parts of the process,
endeavors to make improvements upon the art, or to under-
stand it better than before; such a person, in my opinion, is

a philosopher, or a man of thought, study, and invention. Even in medicine, the same distinction may, I think, be very properly made. The physician who only practises what he has learned, treats his patient as we say *secundum artem*, and gives himself no further trouble, should be reckoned an artist: but, when he bestows uncommon attention and study upon the diseases he has occasion to treat, endeavors to understand them better than ordinary, or to improve the method of curing them, he, in so far, is certainly one of the most useful philosophers....a medical philosopher.*

It may be objected, perhaps, that I use some freedom with common language in this manner of applying the term philosopher, when I do not confine it, as is commonly done, to men of great learning and retirement, but apply it to any man who endeavors to acquire knowledge, or thinks and reasons upon any useful subject. In this sense, the term, it may be said, will apply to a plain farmer, if he only studies the construction of his plough, and how far it is adapted to produce in the best manner the effect for which it is intended, and perhaps endeavors to improve it. And, in so far as he does this, I have no scruple to reckon him a philosopher ; a rustic one, he may possibly be thought, but a more useful one than many who think the title indisputably theirs. Men of great learning and retirement often contribute little or nothing to the progress of improvement. They spend their time in learning and admiring the inventions of others, without ever

* Does not the distinction between the philosopher and the artist consist purely in this : That the latter employs with success certain processes and manipulations with the subjects of his art, without being able to give any other reason for his proceeding but that he was so directed to act, and has always found these precepts completely effective ? And, in communicating this art to another person, he merely inculcates certain precepts which, he tells him, will never fail ; while the philosopher perceives, by his knowledge of the laws of nature, how every operation is efficient, and must, in conformity to those laws, be followed by the desired effect, and by no other. The artist may, like Wedgewood, improve his art greatly, by dint of many trials, either made at random, or by reflecting on his former practice : but the philosopher can improve the art, and even invent new arts, by proceeding from fixed principles and applying them with propriety ; and he sees clearly the reason of every effect in the nature of the means employed. EDITOR.

proposing a new thought of their own, or ever discovering one useful power in nature.

If this distinction, therefore, between science and art be allowed me, I would not define the system of knowledge which I propose to comprehend in these lectures, by saying that it is an art. An art is a set of rules and directions for the use of an artist, who, by practising them, is enabled to obtain certain productions which are the object of the art. A science is a body, or system of knowledge; and chemistry, as now generally studied and taught, is undoubtedly a science, which, though it has given origin to numerous arts, is distinct from them all, as you will more easily perceive when we are farther advanced in this course.

But, setting aside this impropriety in some definitions of chemistry, what has been added to distinguish it from other branches of knowledge is not less exceptionable. The object of it is commonly said to be the resolution, or division, of compounded bodies into the principles of which they are composed, and the production of new compounds, by combining bodies which were before in a separate state. This will be found to be the meaning of Dr. Stahl's definition, though delivered in terms that are a little abstruse. And the same distinction is expressed more clearly in Mr. Macquer's, which, as being one of the latest, may be taken as an example.*

But, when this manner of defining chemistry is better considered, it will be found improper upon several accounts;.... as, first, in a great number of the experiments or operations which the chemist performs, he neither divides compounded bodies into their principles, nor combines others which were separated before, nor has he either of these ends in his view in many of his inquiries. To form an idea of the sort of knowledge which he possesses, and of the manner in which his mind is commonly employed, we need only to read the chemical history of any particular substance, as contained in the ordinary systematical books; or we may look to the ac-

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* La Chymie est une science dont l'objet est de connoitre la nature, et les proprietiés des tous les corps, par leurs analyses et leurs combinaisons. *Dict, de Chymie.*

count of some late chemical enquiry into the nature of a new-
ly discovered fossil, or other object, which had not been much
examined before.   If we observe what particulars have drawn
the chemist's attention, and what points he has been anxious
to inquire about, we find that he first examined its external
appearance, in order to form some judgment of its nature,
by comparing it with a number of other bodies he already
knows.   After examining its external appearance, he straight-
way inquires whether or not it resembles them in other res-
pects.   He puts it into the fire, to learn if it be inflammable,
or whether its color, texture, weight, or other qualities, can
thus be changed; whether it melts or assumes the form of
vapor, and at what particular degree of heat it undergoes
these changes.   He mixes it with a variety of other bodies,
solid or fluid, in order to learn whether or not it dissolves in
the fluids, or any of them, and if it does, whether any changes
of color, odor, or other qualities, are thus induced.   And
many of these last trials are diversified by the application of
heat at the same time.   Now, I aver that we should give a
very improper account of his employment and object on this
occasion, if we define it by saying that it is only to compound
the fossil with other bodies, or to separate the ingredients of
which it is composed.   When he examines the facility where-
with it is melted, or converted into vapor; the particular
manner in which it burns, if it be an inflammable body; the
changes of color, texture, consistency, odor, or other qualities,
induced in it by the multiplicity of his experiments....he cer-
tainly is not studying to decompound the subject of his in-
quiry, or to combine it with other bodies.   If it should hap-
pen, during the course of his research, that any appearances
present themselves which give him reason to think that it is a
compounded body, separable into substances of different
kinds, he will investigate its nature in this respect also, and
will apply various means which he knows to be effectual in
separating the ingredients of other bodies.   But although he
should thus find it to be a compounded substance, and be able
to seperate the different ingredients of which it is composed,
he will consider this discovery as only a part of the chemical

history of this new object, the nature of which he was thus investigating.

Dr. Boerhaave appears to have perceived the impropriety of the definitions which had been attempted by others, and proposed one in which he gave a new distinction, or character, of chemistry. We find his idea of chemistry the most clearly expressed in some passages of his *Methodus Studii Medici* and of his *Elementa Chemiæ*. He aims at it too in the definition which he gives in the beginning of his elements; but that definition is expressed in such general terms, that it cannot be understood by itself. In his *Methodus Studii Medici* we find his meaning fully expressed. Recommending the study of chemistry to the physician, he says that it is a principal, or rather the most important branch of the study of nature; and he characterises it by adding, that, whereas what is commonly called natural philosophy is the study of the more general qualities and affections of matter, such as external form, number, bulk, weight, motion, rest, chemistry is the investigation and study of the more particular qualities, the qualities peculiar to each distinct kind of matter, many of which qualities are of the greatest importance to mankind: such as the polarity of the magnet, and its power to attract iron and steel; the disposition of steel to receive the same powers from the touch of the magnet; the exploding quality of gunpowder; the softness and flexibility of lead; the hardness and strength of iron; the density and durability of gold; the poisonous quality of arsenic; the nourishing quality of wheat and other grains; the sweetness of sugar and of honey; and innumerable others. These are all qualities which are found in those particular bodies, but not in bodies in general; and Dr. Boerhaave thought it to be the characteristic of chemistry that it was the science which treated of these particular qualities of bodies.* I find myself, however, under the necessity of dissenting from the Doctor upon this point for the following reasons.

* See Method. Studii Medici, 1751, page 133. Elementa Chemiæ, Paris, 1753, page 45.

If it be made the distinction of chemistry, that it is employ-
ed in considering the particular qualities of bodies, in opposi-
tion to those general ones which belong equally to all kinds of
matter, a great many things will be comprehended among the
objects of this science which in reality do not belong to it,
but to other sciences: and the bounds of it will therefore be
extended in some respects a great deal too far. To give ex-
amples, we may begin with some of those very qualities which
Dr. Boerhaave has quoted as objects of chemistry, or of the
chemist's attention. The attracting and polar qualities of the
magnet and of steel, when rightly prepared. These are qua-
lities particularly belonging to these bodies; and, for this
reason, Dr. Boerhaave has cited them as qualities, the study
and knowledge of which employed the attention of the che-
mist more than of any other philosopher, and which he could
best explain. But it is certain that he has done this impro-
perly. The chemists have not paid any particular attention
to the polarity of the magnet, or the nature of its attractive
power. They avail themselves of this power indeed when
they wish to purify iron filings; and they take some notice of
these qualities when they treat of that particular ore of iron
called loadstone, or magnet. But they have studied the na-
ture of this stone, not on account of these qualities, but as it
is an ore of iron. The books, in which you will find the at-
tracting qualities and polarity of the magnet, and of magneti-
cal steel, most fully considered, are not the books of chemistry,
but the books of experimental or mechanical philosophy. The
same may be said of the electrical qualities of the tourmalin,
the study of which belongs to the electrician; as also of the
qualities of Iceland crystal with respect to light, to which the
attention of the optician is directed, not that of the chemist.
But besides these, we have still a long list of particular quali-
ties to add, the study of which certainly does not belong to
the chemist. These are the virtues of all the remedies em-
ployed by physicians to act on the body; the purgative quality
of rhubarb, the emetic one of ipecacuanha, and so on of all
the rest. These are certainly particular qualities; but no
person will say that it is the chemist who studies them, or

ought to study them the most. It is the physician who ought
to be best acquainted with them; and the study of them has
always been considered as a necessary branch of his science.
Dr. Boerhaave has been so little aware of this, that he has
mentioned some of those very qualities, by which different sub-
stances act on the human body, as objects of chemical study;
such as, the poisonous quality of arsenic. " Sed chemia in-
" venit er sua experimenta vim deleteream inesse huic ar-
" senico." But surely this is without foundation. I will be
bold to say that there is no chemical experiment from which
it could be inferred that arsenic is poisonous.

Thus it appears that Dr. Boerhaave's definition takes in
many things which do not belong to this science. I will fur-
ther aver, that it leaves out many things which do really be-
long to it. The various kinds of matter have many resem-
blances to one and other in their disposition to be affected by
heat. They have *general qualities*, or a *general nature*, with
regard to heat, which become objects of the chemist's atten-
tion and study, but which Dr. Boerhaave excludes by his de-
finition. They have also a few *general qualities*, which they
show in their manner of mixing with one another, and
which are undoubtedly objects of the chemist's attention, but
which are also thus excluded by Dr. Boerhaave.

I find myself, therefore, under the necessity of rejecting
this definition, or distinction of chemistry, and all others I
have hitherto met with. Even Mr. Fourcroy's, though lately
proposed, does not appear to be well chosen or imagined. He
defines chemistry " to be a science which teaches the know-
" ledge of the intimate and reciprocal action of all the bodies
" in nature on one another." To this definition it may be ob-
jected, that it requires much explanation. The intimate and
reciprocal action are terms which cannot be readily under-
stood. They would themselves need new definitions to ex-
plain them, and to limit their meaning, and they might be the
subjects of much disputation.*

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* When motion is given and taken away, by the collision of bodies, they
certainly act intimately and reciprocally on each other: yet the study of this
action is foreign to chemistry. Some further restriction is therefore neces-
sary ere this definition be suitable to our study.

Since I find reason, therefore, to reject the definitions of others, it is incumbent on me to offer one which may be free from the objections which I have stated against them. And this I shall venture to attempt in these words :

CHEMISTRY *is the science or study of those effects and quali-ties of matter which are discovered by mixing bodies variously together, or applying them to one another with a view to mix-ture, and by exposing them to different degrees of heat, alone, or in mixture with one another, in order to enlarge our know-ledge of nature, and to promote the useful arts.*

Or, in fewer words, *That the chemist studies the effects pro-duced by heat and by mixture, in all bodies, or mixtures of bo-dies, natural or artificial, and studies them with a view to the improvement of arts, and the knowledge of nature.**

This, in my opinion, takes in all that is proper to chemis-try, and inseparable from it, and at the same time leaves out every thing that does not belong to the science.

It may, perhaps, be thought to leave out a great deal too much ; and strangers to it may find difficulty in conceiving how any great variety of curious or useful knowledge can be comprehended under this definition.

But when we shall have had opportunities to enter more fully into our subject, I have no doubt of its appearing clear, that not only an attention, to the effects of heat and mixture, with the view I mentioned in the definition, is the characteristic of the chemical philosopher, but that the study and science thus defined lead to an inexhaustible fund of interesting or useful discoveries.

To form some idea of the views which this science opens, and the objects which it presents to the mind, we must reflect a little upon the subject of heat. Whatever that is which we call heat, and in whatever manner produced, it is certainly the chief material principle of activity in nature. Upon its gen-tle action, as confined within the bounds prescribed to it in

* May it be thus defined? Chemistry is the study of the effects of heat and mixture, with the view of discovering their general and subordinate laws, and of improving the useful arts.

this part of the universe, depend the production and preser-
vation of all animal and vegetable life.   Take away heat, to a
certain degree, and they must all perish : a total stop would
be put to all the operations of nature.   But beside this exten-
sive influence, by which it supports action and life in this
great system of beings, the manner in which the various par-
ticular kinds of matter are affected by the different degrees of
heat, is a subject of inquiry which we shall find most fertile of
surprising and useful discoveries

Nor are the effects of mixture less various or extensive
than those of heat.   It is a field of inquiry to which we can-
not see any bounds.

But, further, in order to apprehend better the nature and ex-
tent of this science, it is necessary to observe, that the chemist
does not confine his attention to the phenomena which he dis-
covers himself by intended experiments : the knowledge he
has acquired directs his attention to all similar phenomena
and operations which happen spontaneously, or in the course
of nature, and enables him to reason better concerning these
than any other person.   Thus, various mixtures of subtile ex-
halations are oftentimes formed in the air, some of which pro-
duce meteors of different kinds, which draw our attention by
their striking appearance.   Others, though not perceptible to
the sight, or other senses, become but too manifest by their
pernicious or fatal effects on various animals.

A perpetual succession of mixtures and combinations is also
going on in the waters of this globe, especially those of the
ocean, and those which flow through the hidden veins and ca-
verns of the earth, where their qualities are often influenced
also by the effects of subterranean heat.

Even the solid parts of the globe are undergoing constant
changes of their mixture and composition, which give occa-
sion to new productions.

And, in animals and vegetables, their nourishment and
growth, and the production of the different fluid and solid sub-
stances of which their bodies consist, depend very much upon
combinations of water with other matters, or upon changes of
mixtures and combinations which had been formed before.

On all these subjects, therefore, an intelligent chemist on account of the knowledge he has acquired of the effects of mixture and heat, is the best judge. He is more ready to understand them, and to reason upon them, than another person. And this has been one cause of the difficulty of giving a proper definition of chemistry. Some of those who tried it thought it was necessary to comprehend in their definition all those subjects concerning which the chemists had attempted to reason; and that all the qualities and phenomena which they had endeavored to explain were proper and necessary objects of chemistry. But this was surely a very great error; for although chemical experiments have thrown some light on the digestion of food in the stomach, we must not therefore consider the study and knowledge of the digestion of our food as an article which belongs in particular to chemistry: the branch of science to which it especially belongs is the study of medicine. Some of the chemists have pretended to explain the virtues of the most of the remedies employed by physicians, supposing them to depend on certain proportions which they contained of the imaginary principles, salt, sulphur, water, earth, and others. Must we on this account admit that the study of the virtues of remedies does not belong to the physician but to the chemist? They also attempted to explain the phenomena of thunder and lightning, by supposing that nitrous and other vapors were elevated into the atmosphere, and acted there on one another as we see them act in exploding compositions. Shall we, therefore, consider the study of these meteors as a necessary part of chemical study or knowledge, although later discoveries have shewn that the study of them belongs most particularly to the electrician?

The discoveries of the geometer have enabled him to explain many things in mechanics, in optics, in astronomy, and in the structure of the bodies of animals; but we are not, therefore, to say that all these branches of knowledge belong to geometry, and make a proper part of it. The only study which belongs in particular to the geometer is that of the properties and relations of lines, figures, and quantities. The knowledge he acquires, by the study of these, proves a source

from which we derive many clear explications of obscure points, and the solution of the most intricate questions in other sciences; but all these other sciences, as they are distinct from one another, are likewise so from the study which principally occupies and characterises the geometer.

Chemistry, therefore, must be understood to have the same relation to many other branches of knowledge that geometry has. It supplies principles by which many otherwise dark and intricate points in these other sciences are clearly explained; and thus throws much light on many of the great operations of nature. But if we desire to form a just judgment of the nature and limits of this science, we must not consider these applications of it to the support and illustration of others, as essential parts of chemistry, or as parts which must be comprised in its definition. In defining this science, we must confine our attention to that body of knowledge which principally occupies and engages the chemist, and by the means of which he is enabled to throw light upon other sciences more or less allied to his own.*

What is above stated, will, I hope serve to convey a precise idea of the extent and nature of chemistry, and at the same time show that the chemist does not confine his attention to mere facts, but that his study is speculative and philo-

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* Is it not by a strict attention to the limits allowed by this definition that we most clearly discover, in the organised bodies of this globe, principles of mutual relation and action altogether different from any discovered in the purely chemical phenomena? The hemlock and the pea spring from their respective seeds in the same water; the alum and the nitre shoot from their respective crystals in the same brine; and both go on increasing with considerable similarity. But, in the first example, a certain germ is necessary, and it grows by intus-susception, and subsequent assimilation of heterogeneous matter, and protrusion of a new substance. In the second, any particle whatever of each of the two salts may be the incipient crystal; and the apparent shooting is not a growth, nor a protrusion, nor the production of a new substance, but is an apposition from without of a substance already present in the surrounding brine. The first is effected by a principle of *growth*, or *vegetable life*, producing two perfectly distinct individuals. The second exhibits no such principle; and the result is only a part of a mass, which may increase till it contain all that exists of its kind in this globe. The mutual relations and laws of action are altogether disparate and unlike.

                                                    EDITOR.

sophical science, proceeding like all other such sciences, on the relation of cause and effect.

If it be questioned, in the next place, upon what foundation it has been considered as so extensively useful, we could easily give a list of the most useful arts which have had their origin from the observation of the effects of heat and mixture in various bodies. And as many of these arts employ for their chief materials substances the most familiar to the che-mist, and the nature of which he knows better than any other person, it must be perceived that there is no science more connected with the useful arts, or of greater consequence to their improvement.

In order, however, to see this in a clearer light, let us take a cursory view of the chemical history of vegetables in gene-ral, and observe what particulars have been attended to, and how far the study of these has been useful. In the first place, by an attentive examination of vegetables, or vegetable sub-stances, we have learned that some abound with resin, others with gum; some contain sweet juices, or sugar, and others astringent, or bitter, or acid matter; in others we find oils of different kinds; and in others coloring or tinging materials, which can be transferred to wool, or linen, or other subjects of art; and many contain substances which have medicinal effi-cacy. All of them are either extracted or prepared for use by operations which are entirely or mostly chemical, and which are therefore the foundation and support of a variety of useful arts.

By applying water to vegetables, with a view to mixture, the chemist has learned that commonly something is extract-ed and dissolved by the water; and the application of heat to this water has shown that it commonly dissolves more than without it. By exposing this infusion or decoction to a gen-tle heat, in open vessels, he has found that the water thus gra-dually exhales and leaves the whole, or most of what it had dissolved, in the form of a solid or tenacious mass, applicable to the purposes of medicine; and a variety of others, accord-ing to the nature of the vegetable. While the water exhales, he has often observed an odorous matter to arise with the vapor.

This has suggested to him the contrivance of vessels and in-
struments by which this vapor might be preserved and con-
densed again by cold in the apparatus for distillation. He has
then discovered that the odor resides in a subtile and volatile oil,
which is thus separated and preserved; and the various kinds
of which are useful in different arts, as well as in medicine.

The same operations, repeated with spirit of wine, instead
of water, afford a variety of other products, many of which
are equally useful.

By the simple application of fire, the vegetable is burnt to
ashes, the volatile parts being either dissipated or consumed.
These ashes, being mixed with water, are dissolved in part,
and form a clear ley. When this clear solution, or ley, is de-
canted off, and exposed to heat in open vessels, the water ex-
hales, and a salt remains behind, which has many useful qua-
lities. Being mixed with sand, and exposed along with it to
a violent heat, it produces glass, and is an indispensable arti-
cle in its composition. Upon this invention again depend the
arts of enamelling, and glazing all kinds of earthen ware, the
construction of mirrors, telescopes, microscopes, thermome-
ters, and other philosophical instruments, besides the easy
manufacture of vessels, the most useful and elegant, for many
of the purposes of common life. The same salt properly
mixed with oils, unites with them, and forms soap, and is the
most active ingredient in that useful compound. It is like-
wise, both by itself, and in form of soap, an important material
in the art of bleaching linen, and is often used in the art of
dying, which is entirely chemical.

When the vegetable is exposed to the action of heat, diffe-
rent kinds of steam, or vapor, are observed to arise from it
before it takes fire. This has suggested the condensation of
these steams; and this again gave origin to the art of making
tar, and to what is called the chemical analysis of vegetables,
by which, and by the further action of fire upon them we
learn a most curious and wonderful truth, which, however,
agrees perfectly with what is observed in attending to their
growth and nourishment. I mean that they are all composed
of the same materials or principles, and these very few in

number. Notwithstanding the immense variety among them, or their parts, in point of form, color, strength or hardness, smell, taste, medicinal efficacy, and other qualities, they can all be resolved into a very few principles, or elementary substances, which are too simple to be supposed disguised, and from whose various proportion, combination, and arrangement, therefore, the whole of this amazing variety is produced.

Instead of this outline of the chemical history of vegetables, we might have found an example equally good for our purpose in the history of the metals, the extraction of which from their ores, the refinement and separation from one another, the giving them different degrees of hardness, elasticity, and other properties, by which they become fit for many useful and important purposes, are all examples of the effects of mixture and heat. But to enter fully into any one of these articles would carry us at present a great deal too far.

It may perhaps be thought that I have said too much in representing chemistry as the study which has given occasion to the invention and improvement of so many arts ; and that many of those arts have not received their origin or improvement so much from chemists as from other persons, who, unacquainted with chemistry in general, but being employed in some chemical art, and hoping to find their interest in its improvement, engaged themselves in inquiries and new trials, which conducted them to some useful discovery. But to this I would answer with what I said before, that the nature of a science, and what belongs to it, do not depend on an arbitrary name, or on the extent to which it is known and cultivated by any particular person, or at any particular time. It was certainly a part of chemistry which these artists were cultivating, although they might perhaps be unacquainted with the rest of the science, and did not know that the study and research in which they were employed belonged to chemistry. I can imagine a person living in a part of the world where the name of chemistry is totally unknown, but who, by his genius, taste, and industry, in making experiments, might acquire a great part of the knowledge by which a chemist is at present distin-

guished among us. His discoveries would not be the less a part of chemical knowledge that he did not know the name of this science; nor is it to be doubted that he would deserve the name of a chemist, and would even be considered as a very extraordinary chemical genius.

It is perhaps true that a greater number of improvements in arts have been invented by ingenious men who were artists themselves, than by general and merely philosophical chemists; but this is not surprising. The number of philosophical chemists is small, when commpared with that of the chemical artists. And there are other reasons why an ingenious and inquisitive artist may often discover improvements in his art, especially such as are obvious and easy. The art to which he applies engrosses the whole of his attention; and his interest constantly pushes him on to attempt improvements: that he may vie with his rivals, and better his condition. He therefore becomes, in fact, a chemical philosopher, as well as an artist, with this advantage, that his whole study is directed to one point, while the attention of the general and philosophical chemist is divided among all.

The condition of an artist, therefore, is, in some respects, more favorable to the discovery of some improvements; but it would be incomparably more so, were he to acquire some general knowledge of the sciences connected with his art. His field of knowledge would thus become more ample, and his views more extended; and his invention would have a larger stock to employ itself on, while, at the same time, the general principles of which he would become master would enable him to contrive new trials with better prospect of success, and to understand them more thoroughly when he had made them.

From this it is plain, that an acquaintance with the general principles of chemistry would be of the greatest use and importance to many ingenious and inquisitive artists, whose art is wholly or in part chemical.

These few hints will, I hope, be sufficient to answer the purpose I have at present in view, which is to engage your attention in the study of chemistry. And, as I have now also

explained to you my idea of the nature and extent of this science, you are fully prepared to enter upon it, and to understand the propriety of the plan which is to be observed in this course, and the great lines of which I shall now lay before you.

In the first place, the principal division of our subject shall be into the *more general* and the *more particular doctrines* of chemistry

Under the division of more general doctrines shall be delivered,

1. An account of the more general or universal effects of heat.

2. The more general observations and discoveries relating to mixture.

3. An account of the chemical apparatus or instruments, and the manner of using them, or the chemical operations.

Under the division of the more particular doctrines will be given

A particular account of all the most remarkable bodies, or kinds of matter, which the chemists have studied ; which shall be distributed into a number of classes, and considered in that order which, in my opinion, is best suited to their being easily understood and remembered.*

And lastly, while we thus give an extensive view of the science of chemistry, we shall not neglect the application of it to the illustration and improvement of pharmacy and other chemical arts.†   This is the second great branch of my pro-

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* I trust that you will find that the account which I shall give of their properties is not merely a vast collection of individual facts, each of which must be remembered in itself, but that your own reflections will naturally form those facts into general groupes or parcels, each individual of a class having a common character, which being recollected, they are all remembered. By this voluntary process of your own thoughts, you will find yourselves, at the end of our course, professors of the knowledge of many extensive laws of nature, which regulate all the chemical operations on this globe.   This will be the body of science, founded on observation and experiment, which constitutes the reward of your attention and study.

† The application of Chemistry to arts and manufactures has heretofore been too much neglected by scientific writers. The illustrious author of these Lectures was not inattentive to its importance, and the present

posed plan. It is plain that this will be in our power. For if science be the discovery of the laws of nature, the knowledge of those laws will enable us to foresee what will be the result of any process, and must point out to us, in all cases, the means, and the best means, for producing any desired chemical effect: and here does our science repay, with a liberality unparalleled in any other science, all her former obligations to the arts of life. From them did she borrow the many facts which excited her to speculate; and her occupation has at last enabled her to repay her debts with large interest, while she has grown rich in knowledge almost beyond hope.

edition, it is hoped will be found, in addition, to contain some particulars on this subject not entirely uninteresting. Dr. Black's Lectures were always attended by a considerable number of ingenious artists....an example worthy of adoption.

GENERAL DOCTRINES OF CHEMISTRY.

PART I.

GENERAL EFFECTS OF HEAT.

INTRODUCTION.

OF HEAT IN GENERAL.

THAT this extensive subject may be treated in a pro-
fitable manner, I propose

1st. To ascertain what I mean by the word HEAT in these
lectures.

2dly. To explain the meaning of the term cold, and ascer-
tain the real difference between cold and heat.

3dly. To mention some of the attempts which have been
made to discover the nature of heat, or to form an idea of
what may be the immediate cause of it.

4th, and lastly, I shall begin to describe the sensible effects
produced by heat on the bodies to which it is communicated.

Any person who reflects on the ideas which we annex to
the word heat will perceive that this word is used for two
meanings, or to express two different things. It either means
a sensation excited in our organs, or a certain quality, affec-
tion, or condition, of the bodies around us, by which they
excite in us that sensation. The word is used in the first
sense when we say, we feel heat; in the second when we say,

there is heat in the fire, or in a hot stone. There cannot be a sensation of heat in the fire, or in the hot stone, but the matter of the fire, or of the stone, is in a state or condition by which it excites in us the sensation of heat.

Now, in beginning to treat of heat and its effects, I propose to use the word in this second sense only, or as expressing that state, condition, or quality of matter, by which it excites in us the sensation of heat. This idea of heat will be modified a little, and extended as we proceed, but the meaning of the word will continue at bottom the same, and the reason of the modification will be easily perceived.

All the experience we have relating to this quality or affection of matter shews, that it is the most communicable from one body to another of any quality that we know. Hot bodies cannot be placed in the contact or neighbourhood of colder ones, without communicating to these a part of their heat.

When a lump of hot iron is taken out of the fire, how can we prevent it from communicating its heat to the surrounding matter? Lay it on the ground, or on a stone, it very quickly communicates to them a part of its heat; lay it on wood, or any other vegetable or animal matter, it heats them in a very short time to such a degree as to set them on fire; let it be suspended in the air by a wire, a little attention will soon convince us that it communicates heat very fast to the air in contact with it.

Thus heat is perpetually communicated from hotter bodies to the colder around them, and, while it passes from the one to the other, it penetrates all kinds of matter without exception : density and compactness are no obstacle to its progress : it appears to pass even faster into dense bodies, in most cases, than into rare ones ; but the rare and the dense are all affected by it, and transmit it to others : Even the vacuum formed by the air-pump is pervaded by it. Sir Isaac Newton first discovered this by an experiment. He suspended an instrument for measuring heat in a large glass vessel, and exhausted the air, and suspending at the same time another similar instrument in another glass vessel, equal to the former, but not exhausted, he perceived that the one was affected by the va-

riations of heat as well as the other. (Newton's Optics, Query 18th.)

Much more lately some experiments on the same subject were made by the celebrated Dr. Franklin and some of his friends at Paris. They suspended a hot body under the exhausted receiver of an air-pump, and another similar body, equally hot, in the air of the room near the air-pump, and these bodies being such as to shew exactly the variations of heat that happened in them, it was perceived that both of them gradually lost a part of their heat, until they were reduced to the temperature of the room in which the experiment was made, but that the one which hung in the air lost its heat faster than the one which was suspended *in vacuo*.

The thermometers fell from 60° (Reamur).

	IN VACUO.	IN THE AIR.
to 50°	in 17 min.	in 7 min.
37	54	22
30	85	29
20	167	63

The times of cooling are nearly in the proportion of 5 to 2. This is further confirmed by a set of similar experiments, made by Sir Benjamin Thompson. (Phil. Trans. for the year 1786.)

Sir Isaac Newton thought that such experiments gave a proof that the vacuum of an air-pump is not perfect, but that there is in it some subtile matter by which the heat is transmitted. This opinion probably was founded on a very general association in our minds, between the ideas of heat and matter; for, when we think of heat, we always conceive it as residing in some kind of matter; or possibly this notion of Sir Isaac might be founded on some opinion which he had formed concerning the nature of heat.

There is great reason, however, independently of this experiment, for believing that the vacuum of an air-pump is not a perfect vacuum, and for thinking that there is always some subtile matter, or vapor, present in it; but I can easily imagine, and we shall afterwards see abundant reason to believe,

that heat may be communicated, or pass through a vacuum, or a space empty of all other matter.

In this manner, therefore, and upon all occasions without exception, is heat communicated from hotter bodies to colder ones, when they are in contact, or in the vicinity of one another; and the communication goes on until the bodies are reduced to an equal temperature, indicating an equilibrium of heat with one another.

When we consider this communication of heat from hot bodies to colder ones, the first question which may naturally occur to our mind, is, In what manner have these two bodies acted, the one on the other, on this occasion? Has one of them lost something, which the other has gained? And which of them has lost, or which has received?

The vulgar opinion is, that the hot body has lost something which has been added to the other. And those who have attempted to reason more profoundly on the nature of heat, have agreed with the multitude on this point; and have sup- posed that heat is a positive quality, and depends, either upon an exceedingly subtile and active matter, introduced into the pores of bodies, or upon a tremor or vibration excited among their particles, or perhaps among the particles of a peculiar substance present in all bodies; which subtile matter, or tre- mulous motion, they have supposed to be communicated from the hot body to the colder, agreeably to our general experi- ence of the communication of matter or of motion.

But although many philosophers have thus agreed with the indistinct notion of the vulgar concerning heat, that it is a positive quality, or an active power residing in the hot body, and by which it acts on the cold one; some of them have not been altogether consistent in this opinion. They have not adhered to it, with respect to all the various cases in which bodies of different temperatures act one on the other. They have supposed that, in some cases, the colder body is the active mass, or contains the active matter; and that the warm- er body is the passive subject which is acted upon, or into which something is introduced. When a mass of ice, for ex- ample, or a lump of very cold iron, is laid on the warm hand,

instead of heat being communicated from the warm hand to the ice, or cold iron, they have supposed that there is in the ice, or cold iron, a multitude of minute particles, which they call particles of frost, or frigorific particles, and which have a tendency to pass from the very cold bodies into any others that are less cold; and that many of the effects, or consequences of cold, particularly the freezing of fluids, depend on the action of these frigorific particles. They call them Spi-culæ, or little darts, imagining that this form will explain the acutely painful sensation, and some other effects of intense cold.

This, however, is the groundless work of imagination.

To form a well-grounded judgment on this subject, we must begin by laying aside all prejudices and suppositions concerning the nature of heat and cold, and then propose to ourselves this simple question. From whence do these two seemingly distinct qualities of bodies originally proceed; Where are the sources of heat and cold? It will immediately occur, that heat has a manifest source, or cause, in the sun and in fires. The sun is evidently the principal, and perhaps ultimately, the only source of the heat diffused through this globe. When the sun shines, we feel that it warms us, and we cannot miss to observe that every thing else is warm-ed around us. It is also plain that those seasons are the hot-test, during which it shines the most, as well as those climates which are the most directly exposed to its light. When the sun disappears, the heat abates, and abates the more the longer his influence is intercepted. We must therefore ac-knowledge the sun as a manifest cause, acting on all the mat-ter around us, and introducing something into it, or bringing it into a condition which is not its most spontaneous state. We cannot therefore avoid considering this new condition or heat, thus induced in the matter around us, as a positive qua-lity, or real affection, of which the sun is the primary cause, and which is afterwards communicated from those bodies, thus first affected to others.

But, after having formed this conclusion with regard to heat, where shall we find any primary cause or fountain of

cold? I am ignorant of any general occasion or cause of cold, except the absence or diminished action of the sun, or winds blowing from those regions on which his light has the weakest power. I therefore see no reason for considering cold as any thing but a diminution of heat. The frigorific atoms, and particles of frost, which have been supposed to be brought by the cold winds, are altogether imaginary. We have not the smallest evidence of their existence, and none of the phenomena, on account of which they have been supposed to exist, require such a fiction in order to their being explained.

Some persons, however, may perhaps still find it difficult to divest themselves entirely of the prejudice, that in certain cases, cold acts in a positive manner. Such persons may perhaps appeal to our feelings, which give us a striking proof of the reality of cold as well as of heat. When we touch a lump of ice, we feel distinctly that it has a quality of coldness, as well as that hot iron has the quality of heat.

But let us examine what we mean by this quality of coldness. We mean a quality, or condition by which the ice produces a disagreeable sensation in the hand which touches it; to which sensation we give the name of cold, and consider it as contrary to heat, and to be as much a reality. So far we are right. The sensation of cold in our organs is no doubt as real a feeling as the sensation of heat. But if we thence conclude that it must be produced by an active or positive cause, an emanation from the ice into our organs, or in any other way than by a diminution of heat, we form a hasty judgment. Of this we may be convinced by several experiments. We can, for instance, take a quantity of water, and reduce it to such a state that it will appear warm to one person, and cold to another, and neither warm nor cold to a third; the first person must be prepared for the experiment by bathing his hand in cold water immediately before; the second, by bathing his hand in hot water, or by a feverish heat in his blood; and the hand of the third person must be in its ordinary natural state, while the water with which these experiments are made is of lukewarm temperature. Even to the same person, such water might be made to appear warm, when felt with one

hand, and cold, when felt with the other. We are therefore under the necessity of concluding from these facts, that our sensations of heat and cold do not depend on two different active causes, or positive qualities in those bodies which excite these sensations, but upon certain differences of heat between those bodies and our organs. And, in general, everybody appears hot or warm on being touched, which is more heated than the hand, and communicates heat to it; and every body which is less heated than the hand, and which draws heat from the hand which touches it, appears cold, or is said to be cold. The sensation is in some cases agreeable, and in others disagreeable, according to its intensity, and the state of our organs; but it proceeds always from the same cause, the communication of heat from other bodies to our organs, or from our organs to them. What can we more reasonably expect than that the sensation produced by the intro-susception of the cause of heat, whatever that may be, will be different from the sensation that accompanies its emission from our bodies? The sensations of hunger and repletion are equally distinct.

Besides the uneasiness produced by the touch of very cold bodies, the freezing of water has induced many to believe the existence of frigorific particles. Water, they imagined to be naturally, or essentially fluid, and to have its fluidity in consequence of the round figure and fine polish of its particles; and they thought that to give it solidity, some powerful agent must be employed, which can pervert it from its natural state. They have therefore supposed the existence of frigorific atoms, of angular, pointed, and wedge-like forms, which, being introduced among those of the water, entangle, and fix them one with another.

But the whole of this too is imagination and fiction. We have not the least proof that the particles of water are round, or any good reason for imagining that they have that form. An assemblage of small round bodies, however smooth or polished, would not have the properties which are well known in water; and the supposition, that fluidity is a natural or essential quality of water, is a great mistake, occasioned by our seeing it in these parts of the world much more frequently

fluid than solid. In some other parts of the world, its most
common or natural state is a state of solidity; there are parts
of the globe in which it rarely or never is seen fluid; and the
one or the other state of the substance, as of all other
bodies, depends on the degree of heat to which it is exposed.
Pure ice never melts but when we attempt to heat it above a
certain degree; and if we cool pure water to the same degree,
or below it, we are sure to see it sooner or later completely
congealed.

On these two facts alone, however, the sensation we have
of cold, and the freezing of water, has been commonly found-
ed the belief of the existence of frigorific atoms, among the
greater number of those who have thought proper to adopt
such an opinion.

But some of them have been influenced also by the effect
of salts upon ice or snow. Many experiments have shewn,
that certain salts, or strong saline liquors, if they be added to
ice or snow, occasion these last to melt very quickly, and, at
the same time, to become much colder; in consequence of
which, this mixture of ice and salts is employed occasionally
for freezing many liquids which cannot be frozen by ordi-
nary colds. The liquid which is to be frozen is put into a
vessel, and this vessel is plunged into the mixture of ice and
salt.

These, and a few other facts which we shall afterwards con-
sider, are enumerated by Professor Muschenbroek, among
the reasons which he gives for his belief of the existence of fri-
gorific or congealing particles; but they are not a good foun-
dation for such an opinion; and we shall in the sequel have
an opportunity to explain these facts, without having recourse
to such a supposition.

We have, therefore, reason to conclude, that when bodies
unequally heated are approached to one another, it is always
the warmer or less cold body which acts on the other, and
communicates to it a real something, which we call heat.
Coldness is only the absence or deficiency of heat. It is the
state the most proper to common matter; the state which it
would assume were it left to itself, and were it not affected by
any external cause. Heat is plainly something extraneous to

it: It is either something superadded to common matter, or some alteration of it from its most spontaneous state.

Having arrived at this conclusion, it may perhaps be required of me, in the next place, to express more distinctly this something; to give a full description or definition of what I mean by the word, heat in matter.

This, however, is a demand which I cannot satisfy entirely. Yet I shall mention by and by, the supposition relating to this subject, which appears to me the most probable. But our knowledge of heat is not brought to that state of perfection that might enable us to propose with confidence a theory of heat, or assign an immediate cause for it. Some ingenious attempts have been made in this part of our subject, but none of them have been sufficient to explain the whole of it. This however should not give us much uneasiness. It is not the immediate manner of acting, dependent on the ultimate nature of this peculiar substance, or the particular condition of common matter, that we are most interested in; we are far removed as yet from that extent of chemical knowledge, which makes this a necessary step of farther improvement. We have still before us an abundant field of research in the various general facts or laws of action, which constitute the real objects of pure chemical science, namely, the distinctive characters of bodies, as affected by heat and mixture. And, I apprehend, that it is only when we have nearly completed this catalogue, that we shall have a sufficient number of resembling facts to lead us to a clear knowledge of the manner of acting peculiar to this substance, or this modification of matter; and, when we have at last attained it, I presume that the discovery will not be chemical but mechanical. It would, however, be unpardonable, to pass without notice, some of the most ingenious attempts which have had a certain currency among the philosophical chemists.

The first attempt I think was made by Lord Verulam; next after him, Mr. Boyle gave several dissertations on heat; and Dr. Boerhaave, in his lectures on chemistry, endeavored to prosecute the subject still farther, and to improve on the two former authors.

Lord Verulam's attempt may be seen in his treatise *De for-
ma Calidi*, which he offers to the public as a model of the pro-
per manner of prosecuting investigations in natural philoso-
phy. In this treatise he enumerates all the principal facts
then known relating to heat, or to the production of heat, and
endeavors, after a cautious and mature consideration of
these, to form some well founded opinion of its cause.

The only conclusion, however, that he is able to draw from
the whole of his facts, is a very general one, viz. that heat is
motion.

This conclusion is founded chiefly on the consideration of
several means by which heat is produced, or made to appear,
in bodies ; as the percussion of iron, the friction of solid bo-
dies, the collision of flint and steel.

The first of these examples is a practice to which black-
smiths have sometimes recourse for kindling a fire ; they take
a rod of soft iron, half an inch or less in thickness, and laying
the end of it upon their anvil, they turn and strike that end
very quickly on its different sides, with smart blows of a ham-
mer. It very soon becomes red hot, and can be employed to
kindle shavings of wood, or other very combustible matter.

The heat producible by the strong friction of solid bodies,
occurs often in some parts of heavy machinery, when proper
care is not taken to diminish that friction as much as possible,
by the interposition of lubricating substances; as in the axles
of wheels that are heavy themselves, or heavily loaded.
Thick forests are said to have taken fire sometimes by the
friction of branches against one another in stormy weather.
And savages, in different parts of the world, have recourse to
the friction of pieces of wood for kindling their fires. A pro-
per opportunity will afterwards occur for considering this
manner of producing heat, with some attention.

The third example above adduced in the collision of flint
and steel, is universally known.

In all these examples, heat is produced or made to appear
suddenly, in bodies which have not received it in the usual
way of communication from others, and the only cause of its

production is a mechanical force or impulse, or mechanical violence.

It was, therefore, very natural for Lord Verulam to form his conclusion, as the most usual : nay, perhaps the sole effect of mechanical force or impulse, applied to a body, is to produce some sort of motion of that body. This eminent philosopher has had a great number of followers on this subject.

But his opinion has been adopted with two different modifications.

The greater number of the English philosophers supposed this motion to be in the small particles of the heated bodies, and imagine that it is a rapid tremor, or vibration of these particles among one another. Mr. Macquer also, and Mons. Fourcroy, both incline, or did incline, to this opinion. I acknowledge that I cannot form to myself a conception of this internal tremor, that has any tendency to explain, even the more simple effects of heat, or those phenomena which indicate its presence in a body ; and I think that Lord Verulam and his followers have been contented with very slight resemblances indeed, between those most simple effects of heat, and the legitimate consequences of a tremulous motion. I also see many cases, in which intense heat is produced in this way, where I am certain that the internal tremor is incomparably less than in other cases of percussion, similar in all other respects. Thus the blows, which make a piece of soft iron intensely hot, produce no heat in a similar piece of very elastic steel.

But the greater number of French and German philosophers, and Dr. Boerhaave, have supposed that the motion in which heat consists is not a tremor, or vibration of the particles of the hot body itself, but of the particles of a subtile, highly elastic, and penetrating fluid matter, which is contained in the pores of hot bodies, or interposed among their particles : a matter, which they imagine to be diffused through the whole universe, pervading with ease the densest bodies ; a matter, which some suppose, when modified in different ways, produces light, and the phenomena of electricity.

But neither of these suppositions were fully and accurately consi ered by their authors, or applied to explain the whole of the facts and phenomena relating to heat. They did not, therefore, supply us with a proper *theory* or *explication* of the na ure of heat.

A more ingenious attempt has lately been made, the first outlines of which, so far as I know, were given by the late Dr. Cleghorn, in his inaugural dissertation, published here on the subject of heat. He supposes, that heat depends on the abundance of that subtile fluid elastic matter, which had been imagined before by other philosophers to be present in every part of the universe, and to be the cause of heat. But these other philosophers had assumed, or supposed one property only belonging to this subtile matter, viz. its great elasticity, or the strong repellency of its particles for one another; whereas, Dr. Cleghorn supposed it possessed another property also, that is, a strong attraction for the particles of the other kinds of matter in nature, which have in general more or less attraction for one another. He supposes, that the common grosser kinds of matter consist of attracting particles, or particles which have a strong attraction for one another, and for the matter of heat; while the subtile elastic matter of heat is self-repelling matter, the particles of which have a strong repulsion for one another, while they are attracted by the other kinds of matter, and that with different degrees of force.

This opinion, or supposition, can be applied to explain many of the remarkable facts relating to heat; and it is conformable to those experiments of Dr. Franklin, and of Sir Benjamin Thompson, quoted above. For, wherever there is but a very small quantity of common matter, as in the vacuum of an air-pump, there we may expect to find the matter of heat excessively rarefied, in consequence of its own very great elasticity and self-repellency, which, in this case, is little counteracted by the attraction of other matter.

A cold body, therefore, placed in such a vacuum, is supplied more slowly with heat, or with the matter of heat, than when placed in contact with common matter in a denser state, which, by its attraction for the matter of heat, condenses

a much greater quantity of it into the same space.   And a hot body, placed in such a vacuum, will retain its heat longer than in ordinary circumstances, in consequence of the scarcity of common matter in contact with it, by the attraction of which, its heat would be drawn off more quickly than if there were no other matter present but the matter of heat.

Such an idea of the nature of heat is, therefore, the most probable of any that I know ; and an ingenious attempt to make use of it has been published by Dr. Higgins, in his book on vegetable acid, and other subjects.   It is, however, altogether a suppostion ; and I cannot at present make you understand the application of this theory, or the manner in which it has been formed ; the greater number of you not being yet acquainted with the effects of heat, and the different phenomena which this theory is meant to explain, nor with some discoveries which preceded this theory, and gave occasion to it.

Our first business must, therefore, necessarily be, to *study the facts* belonging to our subject, and to attend to the manner in which heat enters various bodies, or is communicated from one to another, together with the consequences of its entrance, that is, the effects that it produces on the bodies.

These particulars, when considered with attention, will lead us to some more adequate knowledge and information upon the subject....which again will enable you to examine and understand the attempts that have been made to explain it, and put you in the way to form a judgment of their validity.

When we attend to the effects produced by heat in the bodies to which it is communicated, we see that they are very various in the different kinds of matter.   But there are some effects which are produced in all kinds, or in a great variety of bodies, in a similar manner or with such inconsiderable variations, that the similarity of its action is sufficiently evident.   This is true, especially with regard to the simpler kinds of matter, such as water, salts, stones, metals, air, and many others.   These similar effects, produced by heat upon such bodies of the more simple kind, may therefore be con-

sidered as the *general* effects of heat ; and thus distinguish-
ed from many which it produces on certain particular bodies
only.

These general effects of heat are, EXPANSION, FLUIDITY,
VAPOR, IGNITION, or INCANDESCENCE, and INFLAMMA-
TION, or COMBUSTION.

SECT. I....OF EXPANSION.

BY expansion is meant an enlargement of the bulk of bo_
dies, which may be observed when their heat is increased;
while, on the contrary, there is a corresponding contraction
when it is diminished.

Of this effect of heat, we have the most extensive experi-
ence, with respect to all the simpler kinds of matter in nature.
The only seeming exceptions, are a very few bodies, which,
while we vary their heat to make it rise a little above, or fall a
little below a *certain* temperature, suffer, in appearance, some
irregular variations of their bulk, which do not agree with the
general rule ; but these irregularities are special to those bo_
dies, and to *those particular* variations of their heat. When
we expose the same bodies to equal variations of heat, but at
a higher or lower temperature, they are affected in the gene-
ral manner.

It may, therefore, be announced as one of the most general
facts in chemistry, that all bodies are expanded by heat, and
contracted by cold.

It is not necessary to satisfy you of this by a great number
of experiments. You may be assured of the general fact, and
I shall merely assist you to conceive it more distinctly, by a
few examples. To choose these the more properly, we may
remark, that matter appears to us always under three forms,
or under forms intermediate between some of these three.
We have it either, *first*, solid and hard in different degrees ;
or, *secondly*, in the form of a fluid like water, oil, quicksilver,
or the like ; or, *thirdly*, we have it in the form of an elastic
fluid, or vapor, like air or the steam of water. To see an ex-
ample of the expansion of *solid and hard matter* by heat, we
may take a cylinder of iron or brass, one inch in diameter,
and six or eight inches long, and exactly equal in thickness
from one end to the other, the ends being also flat or square
with the sides ; we must also provide a flat ruler of iron, with
a round hole in it, that is one inch in diameter, and that will

barely allow the cylinder to pass through it; and the same ruler must have two projecting parts on the edge of it at a distance from one another, which is exactly equal to the length of the cylinder while it is cold. The cylinder being now made red hot, and tried in this state, will be found too thick to pass through the holes, and too long to fall in between the two projecting parts of the ruler; but, being again cooled, it will be found contracted to its former dimensions.

An example of the expansion of *fluid matter* may be had by putting some water, or oil, or spirit of wine, into a round or oval glass, which has a long and slender neck, and by filling the whole body of the glass only. If the glass be then warmed by setting it in hot water, while the heat penetrates into it, and into the fluid which it contains, this fluid will swell and rise in the neck; and if the heat be again abstracted by means of cold water, the fluid will return to its former bulk.

The third experiment may be made with air, confined in a bladder, and in such quantity, that the bladder, while the air is cold, shall not be fully distended by it. If the air thus confined be gradually warmed, by warming the bladder before a fire, it will expand and blow up the bladder until it be fully distended; and being again cooled, it will be sure to return to its former flaccid state.

Such expansions and contractions, therefore, are always a consequence of the variations of heat in bodies. In whatever temperature of heat we make the experiment, if a body is made still hotter, it is sure to expand, or, if it be made cooler than that temperature, it is sure to contract. If, for example, instead of heating the iron or brass, it be made much colder than at first, with ice or snow, a nice mensuration of its dimensions would shew that it had contracted, instead of expanding; and after it is made red hot, and thereby expanded, were we to make it still much hotter, it would suffer a still greater expansion, more or less, proportional to the heat it had received.

The expansion produced by heat must, therefore, be understood to take place in all bodies, on every occasion when their

heat is increased, excepting only a few particular cases, to be mentioned presently.

There is, however, this difference, when the experiments are made with different kinds of matter, that they do not agree together with respect to the *quantity* of the expansion or contraction which they suffer from the same increase or diminution of their heat. In solid bodies it is in general small, and not perceptible without nice mensuration of their dimensions. In fluids it is more obvious, as is seen in water; and in elastic fluids it is still more remarkable. But in the different species of solids, fluids, or elastic fluids, it is exceedingly different, nor has any circumstance that explains this variety been yet discovered. We cannot, therefore, form a judgment of the rate according to which any particular kind of matter expands, except by making experiments, to compare it with others in this particular. A number of experiments have accordingly been made by different authors, the most accurate of which are those published by Mr. Ellicot in the Phil. Trans. vol. 39, and those by Mr. Smeaton, Phil. Trans. 48, and some by Mr. Berthoud, Essai sur l'Horlogerie, second edition, Paris 1786.

Before we proceed further, it may be here remarked that this effect of heat occasions some bodies to crack, and break in pieces, when they are suddenly heated or cooled. The substances most liable to this accident are such as are otherwise brittle, or which have not either flexibility or a strong cohesion of their parts: such are sulphur, glass, and earthenware, and even cast-iron, which, though it has a strong cohesion of its parts, is liable to split, by reason of its want of flexibility. The manner in which this effect is produced by expansion is sufficiently evident. It is liable to happen in these bodies when the heat is applied suddenly, and to some parts of them only, and when the parts to which the heat is applied are not excessively thin.

When, for example, heat is applied suddenly to a part of a glass vessel that is not very thin, the heat expands that surface of the glass to which it is applied, and continues to expand it more, before it can penetrate to the other surface: as this

last is not yet expanded, the glass is necessarily strained, or
stretched, as if a force were applied to alter its form; and
although it withstands this force to a certain degree, it cannot
long withstand it, on account of its brittleness : it is therefore
split or broken.   And the same must happen to bodies of this
kind, when, after being heated, they are suddenly cooled.   In
both cases the fissure always begins in the coldest side.

If such bodies were flexible, it is evident that their flexi-
bility would preserve them from breaking when suddenly
heated or cooled, as they would easily bear that small change
of their form, which would be induced by the unequal expan-
sion or contraction of their parts ; and accordingly, other
metals which are flexible are not liable to this accident, as cast-
iron is.   But glass, though very valuable to the chemists by
many excellent qualities, is so liable to it, that they often suffer
inconvenience and losses by this defect in glass vessels.   The
way to avoid these inconveniences and losses, as much as pos-
sible, will be pointed out hereafter, in describing the chemical
vessels and other parts of the apparatus.

To other artists, the expansion of bodies by heat has proved
useful on some occasions.   When it is necessary, for example,
to bind pieces of work very strongly with iron bands or hoops,
such as carriage-wheels, or very large vessels employed by
the brewer, or other artists, the purpose is easily attained by
taking advantage of this power of heat.   The iron-hoops are
made red hot, and driven on suddenly, while thus extended
and widened by their expansion, and then they are suddenly
cooled by throwing cold water on them : this makes them con-
tract again and bind the work with such very great force, that,
on many occasions, they make a deep print in it.

Another set of artists finding that this power of heat affect-
ed some nice machines which they are employed in construct-
ing, and prevented these from having the degree of perfection
desired in their effect, have invented contrivances by which
expansion is made to counteract itself, or to remedy those
very defects which it occasioned in such machines.   These
artists are the clockmakers and watchmakers.   It was per-
ceived that the going of common clocks and watches is very

sensibly affected by the variations of heat, occasioning the expansion or contraction of some of their parts which are intended to regulate their motion. In the common clock, the rate of its going is regulated by the length of its pendulum. When the pendulum is shortened, the clock goes faster; when it is lengthened, it goes slower; and as it must necessarily be lengthened by heat, and shortened by cold, a common clock, that goes at a proper rate in a moderate temperature of heat, will go too slow in a warmer one, and too fast in a colder. This has been remedied very ingeniously by different contrivances, of one of which I shall here give the most simple idea.

Suppose that we have two metals, one of which expands or contracts just three times as much as the other, by the same variation of heat. They may, in the following manner, be employed in the construction of a pendulum, that shall neither be lengthened by heat, nor shortened by cold.

From the point of expansion A (fig. 1.) a rod or thick wire, A B, of the less expansible metal, must hang down a certain length. At the lower end it must have a stud, or cross piece, B C, strongly fastened, and projecting a little to one side. On the projecting part, C, of this cross piece, must be erected a pillar, C D, of the more expansible metal. To the top of this pillar, another cross and projecting piece, D E, must be strongly fastened; and, from this last, must again hang down another rod or wire, E F, of the first metal, having the ball of the pendulum at its extremity. And now, if the height of the pillar C D, be one-third of the length of the two rods taken together, the pendulum can neither be lengthened by heat nor shortened by cold. For, by the expansion of the pillar, the pendulum is shortened, or the ball is raised nearer to the point of suspension, because the upper end D of the pillar is more raised by its expansion, than the lower end C is depressed by the expansion of A B; and, on the other hand by its contraction, the pendulum is lengthened, or the ball is lowered; but, while this happens, the two rods, by their expansion or contraction, produce a contrary effect; and the quantity of expansion or contraction is the same in the rods that it is in the

pillar, the greater length in the rods compensating for the
greater expansibility in the pillar. The consequence there-
fore must be, that the length of the pendulum, that is, the
distance between the point of suspension and the ball, cannot
be varied by heat or cold. Accordingly, the clocks made for
the use of astronomers, have pendulums constructed upon this
principle, in which pillars of the more expansible metals are
employed to counteract the expansion of the other parts of
the pendulum-rod.

I already remarked, that there are a few seeming excep-
tions from the general fact of the expansion of bodies by
heat, and their contraction by cold. These are now to be
taken notice of.

The most obvious and remarkable example of such an ex-
ception occurs in water. This fluid, in passing through all
the variations of heat, between the greatest which it can bear,
and a cold approaching to that by which it is frozen, is ex-
panded or contracted like other fluids. But, during its change
from the state of a fluid to that of ice, instead of contracting
by the diminution of its heat, it suffers a remarkable expan-
sion, becoming more bulky by about one-eighth, and that with
a force which is almost irresistible. It is common to see
bottles burst by the freezing of water within them. The ice,
being first formed in the neck of the bottle, shuts it up close ;
and the rest of the water, while it afterwards freezes, is sure
to burst the bottle by its expansion.

But experiments have shewn that this expansion of freezing
water is performed with a force much greater than that neces-
sary for the bursting of bottles. Mr. Boyle found that water,
confined by a moveable plug or stopper, in a strong brass tube,
three inches in diameter, lifted, while it froze, a weight of
74 lb. with which the stopper was loaded (History of Cold).
Huygens burst an old cannon by freezing water confined
within it (Du Hamel Hist. de l'Acad. Roy. l. i. § 2. ch. 1.)
The academicians of Florence burst a small hollow brass
globe, the cavity of which was one inch in diameter, by filling
it with water, which was afterwards frozen : and Professor
Muschenbroeck has computed that the force necessary to the

bursting of this ball must have been equal to a pressure of 27720 lbs weight. In the progress of this experiment, however, it appeared that the expanding force of this quantity of freezing water had not power to burst a stronger ball (Muschenb. Tentam. Florentina, p. 135). See experiments on the expansive force of freezing water, made by Major Williams at Quebec, in the years 1784 and 1785, Phil. Trans. of Edin. vol. ii.

This strong expansive force in freezing water explains many things that happen in frost, or are consequences of it; such as the bursting of water-pipes, the raising of pavement, the increase of fertility in the soil, and the splitting of trees, and even of rocks in some cases. The gradual decay and demolition of neglected buildings also, and of the more elevated and rocky parts of the earth's surface, are promoted by the same cause. The bursting of water-pipes happens when they are too much exposed to the cold, or not sufficiently covered with earth, or other matter, to preserve the water in them from freezing. The raising or loosening of the stones in the pavement, is occasioned by the freezing of the humidity contained in the sand or earth in which the stones are imbedded. The increase of fertility in the soil is a consequence of the disunion of all the parts of it, which are liable to cohere too strongly together, and to give it a degree of toughness and density not penetrable by the fine fibres of the roots of plants; but, while the humidity which is in such soil is changed into ice, being every where interposed between the particles of earth, it disunites these by its expansion, and, when the thaw comes, the whole mass is much less coherent and more easily penetrable.

The other effects of frost above enumerated, some of which are principally observed in very cold countries, all depend on the same circumstance, the expansion of freezing water, or watery fluids, in the pores, or crevices, or natural cavities, of various bodies.

As this expansion of freezing water has long been perceived, some attempts have been made ·to explain it, or to

discover the cause of it. It appears at present to depend on two causes.

One of these is the extrication of the air which water contains, combined with it, in a dense non-elastic state. This air can be extricated from the water, by the use of the air-pump, and by heat, and, when thus separated, always assumes the elastic form of common air. We can also perceive that it is separated from the water during its congelation. While water is congealing, numerous bubbles of elastic air are formed within it, some of which rise to the surface, and escape, so long as it is not entirely covered with ice; but those produced after this happens, are entangled in the ice, and form those numerous cavities commonly seen in it. A mass of ice, therefore, with these cavities in it, must be more bulky than the mass of water from which it was made. And this, accordingly, is one cause of the expansion of freezing water in ordinary circumstances. But experiments have shewn that it does not depend on this cause alone. Water has been deprived of its air, by the air-pump, and by heat, as much as possible, and yet, when frozen, was sensibly expanded. Mr. Mairan, who wrote a treatise on ice, is of opinion, therefore, that another cause contributes to the expansion of water in freezing: and this other cause which he endeavors to ascertain, is a strong tendency of the parts of the water, to arrange themselves into ranks and lines, which cross one another at angles of 60 and 120 degrees. He proves the existence of such a tendency in the parts of water, while they concrete into ice, by many examples of it. It may be perceived in water freezing in a common bason, or other such vessel; the ice first formed, is in oblong, slender, pointed concretions, at the surface of the water, and adhering to the side of the bason by one end. While these increase in length and size, others shoot out from the edges of them, at the angles above mentioned; and from these a third order, proceeding from the sides of the second, may perhaps be observed. Other similar concretions, or thin plates of ice, are also formed, which point downwards; and the water, after being traversed by a multitude of these spicular concretions and plates of ice,

comes at last to be totally frozen. Now these appearances shew plainly, that the parts of the water are disposed to cohere together in one particular manner, and to assume one arrangement in preference to others, when they concrete. Were they equally disposed to unite in every way, and in every direction, the ice first formed would be a smooth crust of equal thickness, adhering to the sides or internal surface of the bason ; and, in the farther progress of the congelation, this crust of ice would increase in thickness, until the whole water were changed into ice. Another proof of this tendency of the parts of water to arrange themselves in a particular manner, may be had by examining the flakes of snow, while it falls in very cold weather. These are evidently formed by the concretion of the parts of water into ice in the atmosphere, where there is no obstacle to prevent their concreting together, in any manner to which they are the most disposed. When the weather is so cold that we can examine these flakes of snow before any part of them melt, we find them generally composed of a number of fine needles, or spicular concretions, sometimes irregularly collected together, but, at other times, joined into regular plane figures, resembling a star of six rays, the angles between which are exactly equal. On many occasions, the rays of these starry figures have had small branches issuing from their sides, at angles equal to those which the rays themselves formed with one another, and have been otherwise compounded and varied (Muschenbroeck Phys. de Meteoris). Some authors have concluded, from these figures, that such snow was not formed from pure watery vapors, but from vapors or clouds which contained an admixture of saline, or other particles, which gave the disposition to form such figures. But this opinion is not consistent with the experiments that have been made with snow, which, when collected pure, and melted into water, gives a water purer than any other natural water. These forms of the snow must, therefore, be the effect of a property natural to pure water, and they are most regular when the snow falls in very cold weather, and with little or no wind. The atmosphere being then calm, does not hinder the water, or

watery vapors that are in it, to concrete in the manner to which they are the most disposed.

To understand more distinctly how the parts of water may have this tendency to concrete in this particular manner, we may imagine that it depends on some degree of polarity, with which the attraction for one another is attended. There is no doubt that the immediate cause of their concretion is their attraction for one another; and if we suppose that this attraction is modified by some sort of polarity, which disposes the parts of the water to cohere together, by some of their sides in preference to others, they will naturally form rectilineal figures and spicular masses, joined together at certain angles, such as would be formed by a great number of little balls of cork, if a small steel magnet were thrust into each of them, and the whole of them thrown into a tub of water. But, whatever may be thought of this supposition, there is no room to doubt, that one reason of the swelling of water, when it is changed into ice, is, that the parts of it enter suddenly into a mode of arrangement and connection, different from that of the parts of the water in its liquid state, and which requires more space, in the proportion of about nine to eight.

I must here observe that this exception to the general contraction of simple substances, by the diminution of their heat, is not confined to the mere passage of water into the solid form of ice, but begins to be observed while the water is yet a small degree warmer than ice. It ceases to contract, and while cooling down to the temperature of ice, it expands about $\frac{1}{1712}$th of its bulk. This was first observed (I think) by Mr, Baumé of the French Academy, and is mentioned by him in the accounts which he gives of his hydrometer, and of distillation. Mr. de Luc has examined this matter with still more attention; and Count Rumford has pointed out, with much ingenuity, some important consequences of this singularity, in the great operations of nature. (See Count Rumford's essays, vii. 281, &c.

The only other examples of exception from the general fact of expansion by heat have been observed in fusible iron, and one or two other metals; and they are very similar to

that of water above described. These metals are expanded
by heat, and contracted by cold, in every variation of heat to
which they are commonly exposed. But when we heat them
so much as to make them melt, the metal, while melting, in-
stead of being expanded by the heat then communicated to it,
is sensibly contracted, and forms a fluid denser than the solid
was immediately before; a consequence and test of which is,
that any small portion of the metal that yet remains unmelted,
swims at the surface of the melted metal, as ice does in wa-
ter. This singularity is observed in iron, antimony, and one
or two more of the Brittle metals. In these several cases,
certain other appearances present themselves similar to those
observed in the case of water. The parts of the metal evi-
dently assume a particular arrangement, while they are con-
gealing, in consequence of which they concrete into a num-
ber of oblong, spicular, or angular masses, or branched figures,
very conspicuous in antimony, bismuth, and zinc, when they
are broken, and perceptible in cast-iron also. We may there-
fore suppose that the attraction by which their parts are made
to cohere is also attended with some degree of polarity, which
forces them to assume a determined relative position, and a
new and more bulky arrangement, in passing from a state of
fluidity to that of solidity.

Having now described the few exceptions from the gene-
ral fact of the expansion of bodies by heat, and their contrac-
tion by cold, it is proper to mention a remarkable circum-
stance with which it is attended. The increase of bulk pro-
duced by heat is in many cases so considerable, that it might
naturally produce a supposition that the weight of the body is
also increased. But, when we examine the fact by experi-
ment, we find it quite otherwise.

Some experiments for this purpose are circumstantially re-
lated by Dr. Boerhaave in his Elements of Chemistry. They
were made with a mass of iron, of the weight of five pounds,
and repeated afterwards with other metals, which being weigh-
ed red hot, and after they were again cold, never shewed any
variation of their weight, but what was so trifling and uncer-
tain, that the Doctor concludes there was not in truth any varia-

tion, but only small appearances of it, proceeding from errors or mismanagement of the balance.

Professor Musçhenbroeck, however, was not satisfied with these experiments of Dr. Boerhaave. He was strongly in-clined to believe that heat is ponderous, or produced by a ponderous substance. It appears also that Mr. Buffon thought he could shew by experiments that a body is heavier when it is hot than when it is cold. But similar experiments being made more lately in England, by Dr. Roebuck, (Phil. Trans. vol. 66.) and also by Mr. Whitehurst (ibid.), who is distin-guished by his accuracy and ingenuity in the construction of very nice and delicate balances, they disagreed entirely with those reported to have been made by Mons. Buffon. Dr. Roe-buck made his experiments with the most scrupulous accuracy, and on considerable masses of iron, iron-scales, copper, and sil-ver; and Mr Whitehurst made his with a small mass of gold, weighed with an exceedingly nice and delicate balance. In all these experiments the bodies appeared heavier cold than when they were hot; and one reason for this is obvious: the hot body when placed in one of the scales of the balance, heat-ed and rarefied the air over that scale, which being therefore less pressed down by the air over it than the other scale, this last preponderated. More recent experiments, by Dr. For-dyce, in which this cause of error was ingeniously avoided, concur to shew that bodies become heavier, though in a very small degree only, not by the increase, but by the diminution of their heat. These experiments are to be considered in the sequel.

It is therefore evident, that if heat depends on the pre-sence of a subtile matter introduced into bodies, this matter has not any perceptible degree of gravitation.

But it would be improper to omit the mention of one other fact which Muschenbroeck and others thought to be a strong proof of the opinion that heat is a gravitating substance. This fact was discovered in making experi-ments with the metallic substances. Many of these, when exposed to the action of heat for some time, are gradu-ally calcined, or changed into a calx, which, so long as it

remains in that state, is an earthy-like substance, which has none of the metallic properties. And the fact above alluded to is, that the weight of the calx is greater than that of the metal from which it was produced : 100 pounds of lead yield 110 or 112 pounds of calx, and other metals give a still greater increase of weight in their calxes.

But this increase of weight in a metallic substance by cal cination is no proof that heat is ponderous. When the me_ tallic calx is removed from the fire, the heat leaves it as it would leave any other matter. It must therefore be something else than heat which gives this increase of weight ; and later discoveries have well ascertained that it is air*, a quan_ tity of which is attracted and fixed by the calcining metal, and can again be extricated from it. No metal can be calcined without the plentiful admission of air to it, while it is expos_ ed to the action of heat.

It has not, therefore, been proved, by any experiment, that the weight of bodies is increased by their being simply heat_ ed, or by the presence of heat in them.

This may be thought very inconsistent with the idea of the nature or cause of heat, which I lately mentioned as the most plausible : I mean the notion that heat depends on the abundance of a subtile matter highly elastic, or self-repellent, which easily enters into all bodies, and penetrates them throughout, being strongly attracted by the matter of those bodies.

It must be confessed that the above fact may be stated as a strong objection against this supposition.

Some have attempted to remove it, by supposing the matter of heat to have such a very high degree of subtility and tenuity, that no quantity of it that we can collect together can have any sensible weight. Others have thought it might be the matter which causes the gravitation of other bodies, and that it could not be supposed to gravitate like them, but, on the contrary, might have an opposite ten- dency.

* Oxygen or Vital air.

These attempts to remove the objection are ingenious, but they are not satisfactory. We find too much difficulty in attempting to comprehend them. And this has contributed more than any direct arguments, to confirm in their opinion those, who, with Lord Verulam, assert that heat, or the cause of our sensation of heat, and all the phenomena which accompany that sensation, is not a material substance, transferable from one body to another, but a mere state or condition, in which the matter of all bodies may be found. Yet, notwithstanding this difficulty, I imagine that, as we proceed, you will find yourselves more and more impressed with the belief that heat is the effect of a peculiar substance.

The non-ponderosity of heat being, however, ascertained by all the experiments hitherto made, it is necessarily attended with this obvious consequence, that the density of bodies, in comparison with one another, or what is called their specific gravity, is considerably affected by the variations of heat. As experiments have shewn that each kind of matter has its own peculiar rate of expanding, or differs from others in the quantity of expansion which it suffers, by a certain variation of its heat, it necessarily follows, that when we increase or diminish the heat of a number of different bodies in a similar manner, we change the properties of their densities to one another. In the tables of the specific gravity of different bodies, therefore, these specific gravities are stated as they appear in a moderate temperature of heat, the degree of which ought always to be marked at the head of the table. In higher or lower temperatures, it is well known that the specific gravities of the same bodies would be differently proportioned to one another.

We have now sufficiently considered *Expansion* as an effect of heat, and taken notice of the few exceptions to it.

Our next employment shall be to explain the nature of the thermometer, which was invented in consequence of the discoveries already described, and which has proved exceedingly useful, both in chemistry and medicine, and has enabled us greatly to enlarge and improve our knowledge of heat.

OF THE THERMOMETER.

The history of this invention is a little obscure, and the contrivance of the first thermometer has been attributed to three or four different persons. Sanctorio, who is distinguished by the discovery of what is called insensible perspiration, appears, however, to have the best title to it.* It is uncertain what purpose he had in view when he contrived the thermometer which still bears his name; but it does not appear that the expanding power of heat had been known, or attended to, before his time; and, having observed it in air, he thought of employing it as a measure of heat, by the following simple contrivance, (fig. 2.)

A glass tube, open at one end, is blown up into a ball at the other. The ball is warmed, and the tube is then set upright, with its open end dipped into a small cistern of any colored liquor. When the ball is warmed by the approach of a burning coal, the air in it is expanded by the heat, or becomes more elastic; a part of it therefore issues out at the open extremity of the tube, and rises up through the colored liquor in bubbles. Let the coal be now removed, and the ball allowed to cool again to the common temperature of the place. While it cools, the air in it loses the additional elasticity which it had received from the heat, and the pressure of the atmosphere compresses it into a smaller bulk than that it had while it was warm, and causes some of the colored liquor to ascend in the tube.

* This title however, is somewhat dubious. Drebel, a physician at Alkmaer in Holland, certainly made many thermometers at this time (the beginning of the 17th century) and they were very common in Holland, and even in England, before, Sanctorius was known in these countries. Robert Fludd, also, who began to publish his alchemical writings in 1597, speaks a great deal about thermometers, and of many purposes to which he applied them. Much of this indeed is unintelligible, or is stark nonsense....but he must have had the conception of such an instrument.

A scale of equal degrees, applied to the whole length of the tube, served to divide its cavity into a number of small and equal spaces, and it is now easy to see by how many of these small and equal spaces the bulk of the air was increased in consequence of its being heated, or diminished in consequence of its being cooled. This contrivance, therefore, was found to be a real thermometer, or an instrument which shewed the increase or diminution of heat in the place in which it was kept.

The power of heat to expand air being thus perceived, a curiosity was soon excited to try other fluids, as well as solid bodies, and the result was what has been already delivered. It was discovered that all bodies expand when their heat is increased, and that other fluids might be employed, as well as air, in the construction of thermometers. It was even very soon perceived that air was less fit for this purpose than any other fluid, for this reason, that being in this instrument constantly subjected to the pressure of the atmosphere, acting on it through the open end of the tube, it might be contracted or expanded in bulk, in consequence of variations in that pressure alone, although its heat continued the same. This defect in the air thermometer was perceived by Mr. Boyle, and by the Florentine Academicians, nearly at the same time, and both thought of employing other fluids.

Spirit of wine was the first chosen. It expands or contracts much more than water by the same variation of its heat, and is not liable to freeze like that fluid in violent colds, and it is easily tinged, to make it more visible in the tube.

After spirit of wine had been used for some time, quicksilver was also found to be very fit for this purpose; and whichever of these fluids were chosen, they were put into a glass tube, blown up at one end into a ball, and as much fluid put in as filled the ball and a part of the tube. By this simple contrivance, the smallest expansions and contractions of the fluid are easily discernible, and easily divisible into small parts, or degrees. The extremity of the tube is closed, by

by melting the glass, to prevent any loss of the fluid by evapora-
tion, or any change which might happen in it by the action
of the air. Some authors had scruples at first about this
practice of closing up the tube : it was apprehended that the
air confined in the upper part of it would resist the ascent of
the expanding fluid ; but it is easy to expel a great part, or
the whole of this air, before the tube is closed up; nor is it
at all inconvenient to leave a part of it; experience having
shewn that the expansion of the fluid is performed with a
force too strong to be resisted by the elastic pressure of
the air, especially when that air is rarefied by the abstraction
of a part of it.

In a thermometer constructed in this manner, the fluid can-
not be affected by any cause but heat alone; and the instru-
ment is therefore free from the defect of Sanctorio's ther-
mometer. But for a considerable time after it was brought
to this state, it still remained very imperfect. No method
had yet been thought of for making thermometers agree with
one another. The scale of degrees applied to the tube was
differently constructed and differently applied in every ther-
mometer and experiments made with one could not be usefully
compared with those made with another. The most impor-
tant improvement of these instruments, therefore, since they
were first contrived, was the invention of a method of con-
structing and applying their scales, so as to make them agree
together when they are exposed to the same temperature of
heat, whatever that be.

This was attempted by different methods, more or less suc-
cessful, and some of which were practised for a considerable
time in some of the countries of Europe, until one was dis-
covered preferable to all the rest. This is now become the
universal rule for the graduation of these instruments.

It will therefore be sufficient here to explain this method
alone, the advantages of which, in comparison with the others,
are very well stated by Dr. Martin (Martin's Essays on Heat
and Thermometers.)

It was easily discovered by the experiments of Mr. Boyle,

and others, that some bodies which are liable to changes of their form, in certain variations of their heat, undergo these changes at a certain degree of heat, or when we attempt to increase or diminish their heat above or below that particular degree*. Therefore, if a thermometer be applied to them, while they are undergoing the change, the fluid in it is always reduced to the same state of expansion. We have an example of this in the freezing of water, and in the melting of ice and snow. If, for example, I take a thermometer of quicksilver, and put it into melting snow, taking care that the ball be entirely covered with the snow, the quicksilver will be contracted by the cold, and will descend in the tube ; but it will soon stop and descend no further, but will continue in the same state so long as any considerable part of the snow re-mains unmelted. If I now mark with a diamond, or with a small file, that part of the tube at which the quicksilver stop-ped, and afterwards repeat the same experiment with the same thermometer, however often, and at places and times the most distant, the result will be always exactly the same ; the quicksilver in the melting snow will desend to the same part of the tube to which it descended the first time, and will remain there stationary so long as any considerable part of the snow remains unmelted.

This shews, that melting snow is always equally cold, or has the power to reduce the quicksilver of a thermometer to one steady determined state of expansion or density, which may be called the melting-snow expansion of the quicksilver.

A similar power has been observed also in boiling water†. If the same thermometer used in the above experiments, be now immersed into boiling water, and the water be kept boiling around it for some time, the quicksilver, heated and ex-panded, will ascend in the tube, but it will ascend to a certain height only ; however long or violently the water is boiled, it will not ascend higher. If we now mark with a diamond or file, that part of the tube to which the quicksilver rose, and

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* This was discovered, with respect to the freezing of water, in 1664, by Dr. Robert Hook.
EDITOR.

†This also was the discovery of Hook in 1684.
EDITOR.

afterwards repeat the same experiment, with the same ther-
mometer, ever so often, in places not very high above the sur-
face of the sea, the result in this case also will be always the
same; the quicksilver will always rise to the same point which
was marked the first time, or with very little variations, which
can be foreseen, and allowance made for them. Thus we
learn, that boiling water, when in the same circumstances, is
always equally hot, or has the power to reduce quicksilver to
another determined and steady state of expansion, which may
be called the boiling-water expansion of quicksilver. And,
now, having acquired this information, I can repeat the same
operations with any other thermometer; I can find and mark
the part of the tube to which the quicksilver descends, when
reduced to its melting-snow state of expansion, and the point
to wich it ascends, when reduced to its boiling-water state of
expansion. The distance between these two points on the
tube, will be very various in different thermometers, on ac-
count of their different size, and the different proportions
which the balls and tubes bear to one another; but, being
marked on each instrument, these two points will enable us to
perceive when the quicksilver is at any time reduced to either
of these two determined states of expansion. And, when we
have secured these two corresponding points in every ther-
mometer, we can make sure of a third, provided the tube be
exactly cylindrical, or equally wide from end to end. We
may take the middle point between the two above mentioned;
it will shew when the fluid is reduced to a middling state of
expansion between the two former; and, in this manner, we
can proceed to subdivide the distance between those two
points into any number of parts or degrees, taking care only
to divide it into the same number of parts or degrees in every
thermometer, and to mark or number these degrees in the
same manner in each. The corresponding degrees in the
several thermometers will shew the corresponding states of ex-
pansion in these different intruments. And, if we desire to
measure or mark other states of expansion, above or below
those above mentioned, we can protract the scale above or
below the primary points, by adding to it as many degrees of

the same size as the tube will hold, and marking or number-
ing these also in a similar manner in every thermometer. Or,
we may choose other fixed points for adjusting these parts of
the scale, other ways having been found, by which the fluid of
the thermometer can be reduced to certain determined states
of expanson, different from the above; as, by immersing it
in boiling quicksilver, or into melting lead or tin, and some
other metals, by which the quicksilver is greatly expanded;
or, by putting it into a mixture of snow, with certain salts, by
which it suffers an extraordinary contraction.

This method for constructing the scales of thermometers,
by reducing the fluid to certain determined states of expan-
sion, has great advantages over every other attempted before.
It is easily practised, and as easily with small thermometers
as with those of a larger size, which could not be done by
those other methods. Mr. Boyle first pointed out some of
the facts on which this method proceeds; and Sir Isaac New-
ton put it in practice in the 1701; and afterwards Dr. Hales,
in the 1727, constructed six thermometers for himself, and
took this method to make them agree; after which, the ar-
tists who make thermometers practised it with more or less
accuracy. Dr. Martin, in his essays on heat and thermome-
ters has displayed the advantage of it in the clearest light.

To insure a perfect correspondence between thermometers,
even by this method, there are several circumstances which
must be attended to, besides those mentioned by Dr. Martin.
See in Phil. Trans. for 1788, an excellent paper on this sub-
ject by Mr. Cavendish.

Having thus explained the general principles of the art of
constructing thermometers, the next question is, whether the
degrees of their scales express, or point out, equal differences
of heat? it is plain, that the immediate purpose for which the
scale is applied, is not to measure heat itself, but the expan-
sion produced by heat. The scale of a thermometer divides
the increments and diminutions of bulk into a number of small
and equal parts, that we may see by how many of these parts
the bulk of the fluid is increased at one time, or diminished at
another; but it remains to be considered, whether these equal

increments, or diminutions of bulk, be produced by equal in-
crements, or diminutions of heat. We can imagine the fact
to be otherwise, and that in some parts of the scale, a greater
addition of heat may be required to produce one degree of
expansion, than in other parts. If a string be strecthed,
by suspending a moderate weight to it, and we add one
pound to that weight, we shall make it a little longer; but,
by adding a second pound, we shall not add as much more
to the length of the string as the first pound added; nor will
a third pound produce so much effect as the second pound.
In like manner, we can imagine, that when a thermometer
receives a series of equal additions to its heat, these may not
produce equal increments of expansion; and, therefore, that
equal increments of expansion may require for their pro-
duction increments of heat very unequal among themselves.
This question has been overlooked, or little attended to,
by some of the principal writers on thermometers. It does
not appear to have occured to Dr. Boerhaave; and Dr. Martin
gives very little attention to it. I began to attend to it, and
made an experiment to decide it in the year 1760, and did not
then know that others had thought of it; but I soon learned
that Boyle*, Renaldini of Padua, Wolfus†, Dr. Halley, Sir
Isaac Newton, and Dr. Brook Taylor, had severally given their
opinions or doubts concerning this question; and some of them
have described experiments, by which they thought it was de-
cided. The most simple and satisfactory experiment for this
purpose, is one described by Dr. Brook Taylor, in the 32d
volume of the Philosophical Transactions, published in the
1723. The experiment which I made, and communicated to
the society at Glasgow, was the same with this; and Renaldi-
ni's experiment or process was of the same nature.

Dr. Taylor's experiment was made by mixing cold and hot
water together, by which we can produce various heats, or
temperatures of heat, the differences of which from one
another we can be sure of, independently of any thermometer.
If, for example, we suddenly mix a pound of hot water with

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* Boyle, Abr. i. 580.   † Wolfii Elementa Matheseos, t. i. p. 780.

a pound of cold, the excess of heat which was in the hot water, compared with the cold, being now equally divided between it and the cold water, must produce an excess or strength of heat in the mixture, just the half of that which existed immediately before in the hot water. And, by applying a thermometer to the hot water first, next to the cold, and lastly to the mixture, we learn whether the quicksilver is expanded by different heats, in proportion to the differences of these heats. This experiment has accordingly been made, first by Dr. Brook Taylor in 1723, next by myself in 1760, at which time I did not know (as I said just now) that it had been made or thought of by any other person: and since that time it has been repeated with much care and attention, and varied in the contrivance of it, by Mr. De Luc*, and Dr. Crawford†. To perform it well, attention must be given to two principal particulars: first, it is necessary that the tube of the thermometer be exactly equal in wideness from end to end. This is ascertained, by introducing as much quicksilver into it, before the thermometer is made, as will fill one inch, or one and a half inch of it, and measuring exactly the length of this little column of quicksilver, and then making it slide gradually through every part of the tube, and measuring and marking down its length in all these different situations. It is evident, that those parts of the tube must be of equal wideness, in which the little column of quicksilver is of the same length, and such parts only should be employed in making thermometers.

The second object, of necessary attention in performing the above experiment, is to make allowance for the heating or cooling of the vessel in which the mixture is made. If the warm water is poured into the cold, the cold vessel in which the mixture is thus made, will take from the mixture some of its heat, and make it appear colder than it should otherwise do. If, on the contrary, the cold water is poured into the hot, the hot vessel will impart some of its excess of heat to the more temperate mixture, and thus raise its heat above the degree which should be produced.

* De Luc on the Barometer.   † Crawford on Heat.

To avoid this deception, we must employ the two vessels of the same materials, and of the same size and weight, and then by making the experiment both the one way a the other, and taking the medium of the results, we shall the truth.

The experiment being made with these ⬛ ⬛ tions, the result has shewn, that when the thermo⬛ ⬛ made of quicksilver, the gradual expansions of this fluid, while it is heated slowly, from the cold of melting snow to the heat of boiling water, are very nearly proportional to the additions of heat by which they are produced. There is, however, a little deviation from the exact proportion; while the heat increases, the expansions become a little greater than in proportion to the increase or addition of heat. The equable or uniform increase of heat, therefore, is attended with a small acceleration of the increase of expansion. This is much more remarkable in water, alcohol, and some other fluids, than in quicksilver. In this last fluid, this irregularity is so inconsiderable within the range of heat above mentioned, that it does not deserve any notice in common experiments, and in the ordinary use of thermometers ; but if it should be necessary, on any occasion, to estimate the amount of it in thermometers of quicksilver, and of spirit of wine, it may be collected from the experiments of Mr. De Luc, and of Dr. Crawford.

Here we may remark, that this little disproportion between the degrees of expansion and the degrees of heat, is not at all inconsistent with that supposition concerning the nature or cause of heat which I formerly mentioned as appearing to me the most probable; I mean the supposition that heat depends on the presence or abundance of an inconceivably subtile self-repelling matter, which is attracted by all other matter of the cohering or attracting kind. For the attraction of cohesion, that is, the attraction which the particles of a body have for one another, like all other attractions, is weakened by distance ; and therefore when a body is expanded by heat, and the distance of its particles is a little increased, its attraction of cohesion being thus a little weakened, a further addition of heat to it can easily be imagined to produce more effect in expanding it further ; the distending power of the repellent matter of

heat being rather less counteracted by the attraction of cohe-
sion, than it was at the first.

Such is the decision of this important question concerning
the degrees of thermometers. There is a passage, however,
in Dr. Boerhaave's treatise on fire, which is inconsistent with
what I have now represented to be the fact. He relates, that
Fahrenheit had the curiosity to mix hot and cold water, in
order to see what temperature they would produce. The
result is given as matter of curiosity only. The importance
of such an experiment, as a way to verify the scale of heat,
does not appear to have been thought of; and his account of
the result is so contrary to the fact, and so absurd in itself, that
it is plain he had misunderstood Fahrenheit's account of what
happened. He says, that whatever heat is in the colder water,
that heat, and an equal quantity of the heat of the hot water,
is extinguished, and disappears, and that the excess only of
what is contained in the hot above the cold, remains in the
mixture, and is equally distributed through it. If this were
true, it would be easy, by adding hot water to cold, to pro-
duce a mixture colder than the cold water. Boiling water,
added to water a little less hot, would produce a mixture
colder than ice. Dr. Boerhaave's account of what happens
in this experiment must therefore be disregarded. We have
clear and distinct accounts from others of what is the real fact,
and I have often made the experiment myself.

Thus an important point is gained in our knowledge of heat,
and in the usefulness of thermometers. And, as it is proper, in
order to understand the instruments thoroughly, to know the
imperfections that still attend them, I shall mention what may
be considered as the remaining imperfections of our thermo-
meters.

In the *first* place, they are always more or less slow in
shewing the heat of other bodies to which they are applied.
To understand how this happens, we must remember that the
thermometer indicates immediately and directly that heat only
which is in itself, but from the heat of the thermometer we
judge of that of other bodies to which it is applied. This
depends on the irresistible propensity of heat, which I for-

merly mentioned, to be always communicated from any hotter
body to the colder around, until they come to an equal tem-
perature with one another. If the body we wish to examine
be warmer or colder than the thermometer, no sooner are
they applied to one another than the heat begins to be commu-
nicated from the one to the other, until both are reduced to an
equal temperature; and then, by examining the heat of the
thermometer, we judge of that of the other body. As heat,
however, is not suddenly but gradually communicated in this
manner, we must wait some time after the application of the
thermometer, before we can form this judgment with certain-
ty. The way to diminish this inconvenience, as much as pos-
sible, is to make our thermometers very simple, and of a small
size, it being well known that small masses of matter are
heated and cooled much faster than larger ones*.

A second imperfection attending thermometers is, that
the extent of the variations of heat, in the measurement or
observation of which they can be employed, is limited within
very moderate bounds. One reason of this is, that in strong
or violent heats, the increments of expansion become more
disproportioned to the increments of heat. But setting this
aside, it is well known that the fluid of the thermometer, if it
were exposed to a heat too strong, would be converted into
vapor highly elastic, which would burst the glass; or if, on
the contrary, it were exposed to violent cold, the fluid would
congeal. When we attend to these particulars, we find strong
reasons for preferring quicksilver to all other fluids, for the
construction of thermometers; for besides the advantages at-
tending it, already taken notice of, it bears a much stronger
heat than other fluids, without assuming the form of vapor,
and it also endures very violent cold before it congeals. Lint-
seed oil has also these qualities in a considerable degree, but
the viscidity of it renders it unfit for the construction of small
thermometers. Spirit of wine is still employed for some
thermometers, on account of its great expansion and contrac-

* Besides the other qualities of mercury, which make it fitter for a ther-
mometer, it greatly excells all other fluids in the quickness with which it
attains the temperature of the bodies to which it is applied.

tion, it being almost eight times greater than that of quick-silver. It is the fittest fluid for the measurement of the most intense colds; for, when very pure, it has never been known to congeal; and it may also serve very well to shew the variations of heat that happen in the atmosphere. But it is unfit for being employed, when we examine the heats, that approach to that of boiling water, and those above it. In such heats, the expansions of it become first excessive, and afterwards the spirit produces vapor, highly elastic, which bursts the thermometer.

The immediate application and use of thermometers is, therefore, limited to the measurement of a certain small extent only of the variations of heat.

But, if we desire to compare with these degrees to which the thermometer can be applied, other more intense heats that are far above the reach of the instrument, this can be done by a method, of which an example is given in an ingenious paper on the degrees of heat, by Sir Isaac Newton, in the Philosophical Transactions for the year 1701. He took a lump of red hot iron out of the fire, suddenly exposed it to a stream of cold air, nothing exactly the beginning of the time when it was exposed. As soon as its heat was somewhat abated, he observed with attention the progress of its cooling, or how much heat it lost in equal portions of time. This he did by the use of an oil thermometer, and also by laying upon the mass of iron little bits of different metals, and of mixtures of metals, which he knew by previous experiments required certain intensities of heat to melt them; and he noted the times when the heat of the iron was so much diminished that it ceased to preserve these little masses melted. Thus, having learned what heat was in the iron at different periods of the whole time it required to cool it, he discovered the celerity and progression with which it lost its heat, and consequently could calculate how much it had lost from the beginning of exposition, or how much its heat at that time exceeded the inferior heats through which it afterwards passed.

By such calculations, therefore, we can form an estimate of the intensity of violent heats to which thermometers cannot

be exposed, and thus remedy, in some measure, that defect of the instrument, by which its power is limited in the measurement of heat. I shall hereafter have occasion to mention other methods by which the intensity of violent heats may be measured, or estimated. At present I shall further notice, that this method of Sir Isaac Newton's is useful on many other occasions, beside the measurement of violent heats. It enables us to use the thermometer for measuring transitory heats. Notwithstanding the slowness of it in pointing out the heat of other bodies. There are many experiments in which the heat we desire to measure exists only for a moment at its highest degree. When hot and cold water, for example, are mixed together, a middle temperature is instantly produced; but the middle temperature continues but for a moment; the excess of heat, which the mixture contains above the heat of the surrounding bodies, begins immediately to be communicated to these, and the intensity of it therefore, is very soon diminished,....so soon, that there is not time for the thermometer applied to be raised to that intensity, and therefore to indicate it. But the remedy is easy. We must note with exactness the moment of time at which we know that the heat must be at the highest degree, and immediately after, or at the same time, apply the thermometer. The quicksilver will rise, but after a little while will cease to rise, being now heated to the same degree with the surrounding mixture, the heat of which was abating slowly, while that of the thermometer rapidly increased. After the thermometer ceases to rise, we must continue our observations. It will now fall very gradually and slowly, accompanying, in its progress of cooling, the mixture, or matter, to which it is applied, and will therefore indicate the celerity and progression with which such mixture, or matter loses heat, But when we have learned this celerity and progression with which the mixture cools, it will be easy to calculate how much heat it lost from the beginning of the experiment, and, therefore, to estimate what was the intensity of its greatest heat. Without this method, invented by Sir Isaac Newton, it would be impossible, in many cases, to learn

exactly the full strength of these transient heats; the thermometer, on account of its slowness, never would indicate their full amount.

3dly. It may be considered as a third defect of thermometers, that they do not inform us of the proportions which different heats bear to one another. We cannot say that one heat is double, or triple of another in point of strength or intensity. They inform us only of the differences of heats, or the excess which they surpass one another, or any given degree of heat. The reason of this is, that we are ignorant of the lowest possible degree, or beginning of heat. Some ingenious attempts have been made to estimate what it may be, but they have not proved satisfactory. Our knowledge of the degrees of heat may be compared to that we should have of a chain, the two ends of which were hidden from us, and the middle only exposed to our view. We might put distinct marks on some of the links, and number the rest, according as they are nearer to, or farther removed from, these principal links; but not knowing the distance of any link from the end of the chain, we could not compare them together, with respect to this distance, or say, that one link was twice as far from the end of the chain as another.

The numbers, however, which have been affixed to the degrees of the scales are apt to occasion mistakes on these subjects; but these numbers must be considered as expressive only of the difference, or distance, of the several degrees from some well known degree, such as that of melting snow, or of boiling water. They have no relation to the lowest possible degree or beginning of heat.

As the scale called Fahrenheit's scale, is the one commonly used in the British dominions, it may be first described. The author of it did not choose to begin his scale at the cold of melting snow, as many others have done. He knew there were more intense colds, and he chose to begin at the most intense cold known at that time, and which could easily be produced by art. This degree of cold is produced by mixing together snow and sal ammoniac, or common salt. He

covered his thermometer with a mixture of this kind, and marked the part of the tube to which the quicksilver descended. He next put the thermometer among pure melting snow, and marked also the part of the tube at which the quicksilver became stationary in this situation. Then, dividing the distance between these two points into thirty-two equal parts, he made these the degrees of his scale, and marked them with numbers, beginning at the bottom, and increasing upwards: and he further protracted the scale with degrees of the same size, to the top of the tube, marking these with numbers increasing arithmetically upwards, as far as the scale went. A thermometer, therefore, constructed in this manner, when put into pure melting snow, does not point to the beginning of the scale, but to the 32d degree. When exposed to the air of this climate in the most temperate weather of spring or autumn, it points to the 48th or 50th degree. In our moderately warm summer air it rises to the 64th. In the warm weather of very hot climates it rises to 80 or 90, or sometimes to 100 degrees. And it points to the last, or near to it, when applied to the warmest parts of the human body, or to blood flowing immediately from a vein or an artery; and when it is dipped into boiling water, it rises to the degree 212.

But after this manner of graduating them had been used for some time, it was discovered that there were colds far more intense than that produced by snow and common salt, and that it was necessary for measuring these, to make an addition to the lower end of the scale. This was accordingly done, in some of those thermometers, by adding a series of degrees of the same size with the former; but these added degrees were marked with a series of numbers increasing downwards, and therefore expressing increments of cold, and distinguishing from the degrees of the other parts of the scale, by being called the degrees below o in the thermometers that are made to contain them.

These are the reasons for the particular contrivance of Fahrenheit's scale*.

* Wolfius gives a different account of Fahrenheit's scale. He chose the temperature of salt and snow for the beginning of his scale, and that of

Since it came into use, however, the makers of thermome-
ters, although they apply this scale, do not find the point at
which the scale begins, by making a mixture of snow and salt.
They find the points of pure melting snow, and of boiling
water, and divide the interval into as many degrees as are
contained between these two points in Fahrenheit's scale,
which are 180; and, continuing the scale down below the
melting snow point, they begin the numeration 32 degrees
below that point. Or, if the thermometer be intended for
measuring no greater heats than those of the weather, or the
human body, the fixed points from which the scale is con-
structed are the melting-snow point, and the 96th or 100th
of Fahrenheit's scale, which is found either by the instru-
ment's being held in the maker's mouth for some time, or by
exact comparison with a standard thermometer.

Such is the scale of the thermometers generally known and
used in this country. Those which have come into use in
other parts of the world, are divided and numbered in differ-
ent ways, in consequence of this circumstance, that the instru-
ment, for some time after its first invention, having been
imperfect and ill understood, every person who chose to think,
and to make experiments on the subject of heat, began by
constructing a thermometer, intended to be more intelligible
and exact than those sold by the ordinary makers. They
therefore applied to them various scales, divided, and num-
bered according to some particular method of their own con-
trivance ; and some one of these improved thermometers
came afterwards, in each particular country to supersede
all the rest, and to be established as the one chosen for general
use.

In France, and some other parts of the continent of Europe,
they make use of Reaumur's thermometer, in which the interval
between the cold of melting snow and the heat of boiling water,

boiling mercury for its termination, and he divided the interval into 600
equal parts. This made 32 the temperature of freezing water, and 212
its boiling temperature. He also found that each division of this scale was
$\frac{1}{1192}$ of the bulk of the mercury.

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is divided into 80 degrees only, the numeration being begun at the melting-snow point, with a series of numbers increasing upwards, and another increasing downwards*. In Russia, De Lisle's thermometer is the standard, in which the numeration begins at the point of boiling water, with a series of numbers increasing downwards, and expressing, therefore, degrees of contraction; and the interval between the above point and that of melting snow is divided into 150 degrees, which he supposed to be 10000th parts of the quicksilver nearly. In Sweden, Celsius's is the one established in common use, in which the interval between the melting-snow point and that of boiling water, is divided into 100 degrees, the numeration being begun at the melting-snow point, with a series of numbers increasing upwards.

But, notwithstanding this variety, it is easy to find the degree of Fahrenheit's scale, which corresponds with any degree in the scales of these several thermometers. The two fixed points, of melting snow, and boiling water, bring the regulating points in every one of these scales, each degree between those two points has some point of Fahrenheit's scale which corresponds with it, and the place of which in that scale can easily be found by calculation, or by the use of a diagram, in which these several scales are drawn parallel to one another. Dr. Martin has given an example, containing many thermometers.

The scales of thermometers being now every where regulated by the principles above explained, the instrument is become very generally useful. An universal language is now established concerning the degrees of heat, or it is very easy to translate the language or meaning of one scale into that of another.

* There is, however, a great uncertainty and confusion in the different accounts given by Reaumur of the construction of his thermometers; and the observations which were made by their means cannot be understood without much investigation, in order to discover which of his constructions had been employed. For many years past a thermometer is called Reaumur's, if o be the point of freezing water, and 80 the boiling point....Yet this is very different from *all* of his thermometers.

IMPROVEMENT OF OUR KNOWLEDGE OF HEAT

BY MEANS OF THE THERMOMETER.

1st, *Extension of our Ideas of Heat.*

THE uninterupted contraction of the thermometer by a gradual abstraction of heat, gives us the strongest indication of the latter being the cause of the former. This is frequently accompanied by appearances which are altogether unlike; yet we do not doubt of their being only different effects of one cause. We have no doubt of the freezing of tin being produced in the same way as the freezing of bees wax, nay, as the freezing of water; yet in this last, heat gives place to another sensation, altogether different, namely, the sensation of cold. In common life, we never speak of a diminution of heat in this case, but of an increase of cold. The continued contraction of the thermometer, and the analogy between the freezing of tin, of bees wax, and of water, soon induce the belief that all are caused in the same way, by the abstraction of heat. Thus are our ideas of the operations of heat very greatly enlarged. We now know that its influence is so extensive, and that it is diffused in such quantity through nature, that no mass of matter has ever yet been found so cold as to be totally destitute of heat; for spirits of wine have never yet been frozen, and we believe that it only requires the diminution of its heat to produce this effect. We are sure that it still retained a quantity of heat, and, possibly, a very great quantity. This is proved by covering up such a cold mass with a mixture of snow and certain salts, both of which had been previously made as cold as the mass. This mixture becomes intensely colder than the meterials of it were immediately before, as appears by the applicatian of a thermometer to it, and by the readiness and quickness with which it will freeze many fluids that cannot be frozen by any other means. It therefore reduces the mass involved in it to the same intense coldness, or extracts from this mass a quantity of heat, which we thus learn it had contained before.

We must, therefore, consider the coldest bodies ever observed in nature as still retaining a great deal of heat, the amount of which we cannot define ; the beginning, or lowest possible degree of heat, being yet unknown to us.   Indeed, when we recollect that heat is continually emanating from surrounding bodies, and that other bodies absorb it so much the more greedily as they are colder, we cannot conceive a body altogether void of it, if exposed in the neighborhood of others.

But, to give a clearer view of this subject, it may be proper here to mention a few examples of diminutions of heat, or violent cold, that have been observed.

Dr. Boerhaave, in his elements of chemistry, describes with admiration an experiment made by Fahrenheit.   The most violent cold known at the time when this experiment was made, was that expressed by the beginning of Fahrenheit's scale, or 32 degrees colder than melting snow; it is as much colder than melting snow, as melting snow is colder than the air of our summer weather ; and it may be at any time produced, by mixing snow or pounded ice with common salt.   A natural cold, in which the thermometer had fallen to this degree, had been observed in Iceland, and often in Germany, and some other countries of the continent of Europe. The same degree of cold has also sometimes been observed in Britain; but it rarely happens, and never continues in this island, except for a few hours, perhaps, of an extremely cold night in a very hard winter.   It therefore never produces all its effects ; its duration is not sufficient to let it act with the whole of its force.   But, in those places in which a cold of this degree of intensity continues for some time, it produces very remarkable effects, such as splitting timber, by freezing the juices and humidity that are in the pores of it ; freezing beer, wine, vinegar, and all other fermented liquors.   When stones, or metals, which have been exposed to it, are touched with the tongue, or the softer parts of the human body, these are instantly frozen and mortified, and the principle of life in them is extinguished.   No animals can endure such a cold, except those which are provided by nature with very thick

and warm furs; and man is under the necessity of wrapping himself in a thick covering of the furs of these animals. Few vegetables are able to withstand its power; but they are protected from it by very deep snow, the fall of which always precedes the season of this mortal cold.

Such is the power of this cold, in those countries in which it prevails, during a considerable part of the winter season.

But, in the experiment related by Dr. Boerhaave, Fahrenheit produced by art, a cold which was no less than 40 degrees lower; it was produced by the action of the strong nitrous acid on broken ice, to which it was applied in a very ingenious manner, to make it produce its greatest effect.

Dr. Boerhaave was astonished at the effect of this experiment; but, had he lived a few years longer, he would have heard of degrees of cold observed in nature, which approached to this, which were equal to it, or even exceeded it.

The first example is in the history of a journey to the north end of the Baltic, by some of the academicians of France, at the king's expence, in order to make observations and measurements, for deciding the question concerning the figure of the earth. These gentlemen wintered under the polar circle, where, for some days in the middle of winter, the sun does not rise above the horizon, but only approaches to it, and gives a twilight. In that climate, they found it necessary to use all possible precautions to secure themselves from the dreadful cold which prevailed. They prevented, as much as possible, the entrance of the external air into their apartments; and if at any time they had occasion to open a window or door, the humidity of their breath, confined in the air of the house, was condensed and frozen into a shower of snow; their lungs, when they ventured to breath the cold air, felt as if they were torn asunder; and they often heard the rending of the timber around them by the expansive power of the frost in its pores. In this terrible cold, their best thermometer fell to a degree equal to the 33d below the beginning of Fahrenheit's scale, or the 65th below the cold of melting snow; it is as much colder than the degree of melting snow; as this last is colder than the human body in a healthy state.

But, in other places, and at different times, an intensity of cold has been observed, in severe winters, still greater than this, and fully equal to the cold produced by Fahrenheit's experiment. Such a degree of cold has been observed at the British settlements in Hudson's Bay, and in different parts of the Russian dominions, especially those that are situated far to the north, or distant from the sea, and exposed to winds blowing over an extent of northern continent, such as Kamtschatka, Siberia, Petersburgh, and even Moscow. In some of these places, degrees of cold have been recorded, as occurring naturally, or produced by art, which even appeared to exceed by far the 40th degree below o; such as the colds observed in Siberia by baron Demidoff*, in which the mercurial thermometer has many times fallen to the 120th degree below o, The cold observed by Professor Pallas at Kransnojark, in which his mercurial thermometer fell to 46 degrees below o, in a regular manner; but being then moved, fell suddenly, with a start, to the 80th degree, at which it remained a whole day; and, in the course of this day, he congealed some pure quicksilver, by simply exposing it in an open cup to the cold air (Phil. Trans.) Also the famous experiments made at Petersburgh with snow and nitrous acid, in December 1760, by Professor Braun, in which he also congealed quicksilver, and saw his mercurial thermometer fall, in some of the experiments, to 300, or 400, or 500, and once to 600 degrees below o of Fahrenheit's scale.

But, as soon as those experiments were published, it appeared to me perfectly evident, and I always declared it as my opinion, that the cold produced by Professor Braun, in his mixtures, and that which was observed on some of the other occasions just now mentioned, was not near so violent as it appeared to be by the extraordinary descent of the quicksilver in the thermometer. From many phenomena described in those experiments, I found reason to conclude, that quicksilver, while it congeals, suffers an excessive and irregular contraction, much greater than in proportion to the diminution of its heat; that it is the opposite to water in this re-

* Phil. Trans.

spect; for water, when congealing, is considerably expanded, instead of contracting; its expansion being a singular phenomenon, which does not appear in any other case of the diminution of its heat. But quicksilver, on the contrary, while it congeals, is very much contracted, incomparably more than in proportion to the diminution of its heat, as measured by the thermometer. This was evident, from many phenomena which appeared in these experiments of Professor Braun's, a few of which I shall here point out.

1mo, In the first place, when the mercurial thermometer, which was plunged into the mixture of snow and nitrous acid, fell to those very low degrees, the descent of it was not regular and gradual. It descended indeed slowly and regularly a considerable way, in the beginning of each experiment; but, after this, its descent was irregular, by a rapid motion, or sudden starts of 100 or more degrees at once; and this happened especially when the thermometer was moved or disturbed. Now, it is well known, that in many fluids, when they are sufficiently cooled to make them congeal, the beginning of congelation is promoted by disturbing or shaking the fluid matter. I therefore concluded, that in the above experiments, the quicksilver in the thermometer was first gradually cooled to a sufficient degree, to prepare it for congealing; and that during this diminution of its heat, it contracted in a slow and gradual manner, and that the sudden and violent starts of contraction which afterwards followed, proceeded, not from the farther diminution of heat, but from more or less congelation, which actually happened; for the Professor broke some of these thermometers, and found the quicksilver congealed within them.

2do, Secondly, Professor Braun, in some of these experiments, congealing some quicksilver in a wide and short tube, the upper end of which was left open, saw, that after the quicksilver began to congeal, the upper surface of it became remarkably hollow during the progress of the congelation. But the contraction of the quicksilver which occasioned this hollowness could not proceed from a proportional diminution of heat; for it is well known, and shall be illustrated in the

sequel, that every fluid continues of the same heat, from the moment when it begins to congeal, to that in which its congelation is completed, which generally requires a considerable time. The mass does not become colder, although surrounded with colder matter, until the congelation of it is at last completed. The above excessive contraction of the quicksilver, therefore, after it began to congeal, was a concomitant of its congelation alone, and not of its becoming colder.

3tio, Thirdly, To establish further the idea and belief of this fact with regard to quicksilver, we may remark, that the same thing is observable in other metals, particularly tin and lead. If either of these metals be melted in a crucible, and removed from the fire to cool slowly, they will soon begin to congeal, and, during the progress of their congelation, a considerable hollow, or a number of hollows, will be formed in the upper surface of the metallic mass, although, during the time while these hollows are formed, the congealing metal does not become colder; of which we may be satisfied by immersing the bulb of a mercurial thermometer into the fluid metal, and keeping it there until the congelation is finished. In these metals, therefore, as well as in quicksilver, congelation is attended with an excessive contraction.

The degree of cold necessary for the congelation of quicksilver was not ascertained with precision, until later experiments were made in a more accurate and satisfactory manner, with mixtures similar to those which Professor Braun had used in imitation of Fahrenheit: and these later experiments have shewn that the degree of cold produced in such mixtures is not near so violent as he had supposed it.

The first satisfactory proof we had of this was in some experiments made in the British settlements in Hudson's Bay, by Mr. Hutchins, one of the servants of the Hudson's Bay Company. His first attempts were made in consequence of instructions from the Royal Society; and, as they had not been successful in ascertaining the degree of cold at which quicksilver begins to congeal, I wrote a short letter, at the desire of a friend, giving general directions, in a few words,

for the proper conduct of the experiments. In the former experiments, the mercurial thermometers had been plunged immediately into the cold mixture, as in Professor Braun's trials at Petersburgh, and when the cold became so great as to congeal the quicksilver of the thermometer, the excessive and irregular contraction, which it then underwent, made it quite unfit for measuring the cold. I therefore proposed, that if mercurial thermometers were employed, they should not be plunged immediately into the cold mixture, but into a quantity of quicksilver, contained in a small glass, or wide tube, which glass and quicksilver should be set in the cold mixture. Thus, while the quicksilver in the wide tube was gradually congealing, the thermometer itself, placed in the centre of it, would indicate exactly the degree of cold of the quicksilver congealing around it: nor could the cold congeal the quicksilver of the thermometer, so long as any part of the external quicksilver remained uncongealed. A second set of instructions were sent the following winter from the Royal Society, very full, and very accurately drawn up, and new experiments were made, which were quite satisfactory.

And the result was, that the cold, which is sufficient to congeal quicksilver, exceeds but very little the cold which had been formerly produced by Fahrenheit. It is the 40th or 41st degree below o of his scale*, as measured with a mercurial thermometer, and the 30th, as measured with a thermometer made of the purest spirit of wine, a fluid which does not congeal in any degree of cold that has yet been observed.

This result has been further confirmed by another set of experiments made at Petersburgh, by Dr. Guthrie, to whom I likewise gave a few directions to assist him in the conduct of them†.

We have reason, therefore, to be perfectly satisfied that the degree of cold at which quicksilver begins to congeal, and which is produced by a mixture of snow and strong nitrous acid properly made, is not more than the 40th or 41st degree below o of Fahrenheit's scale. But this degree is as much

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* Phil. Trans. 1783.   † Phil. Trans. 1786-7.

colder than melting snow, as melting snow is colder than the human blood in a violent fever.

Even this degree of cold, however, has been since surpassed by the successor to Mr. Hutchins at Hudson's Bay. He was supplied with proper materials, and very full instructions, by Mr. Cavendish, in consequence of which he made a set of experiments, by mixing snow with vitriolic acid in different states. He thus produced colds, some of which made the best spirit of wine thermometers descend to the 69th degree below o of Fahrenheit's scale.

Thermometers, therefore, by enabling us to measure and compare great and uncommon diminutions of heat, have assisted us greatly to enlarge, as well as to correct, our ideas of heat, to perceive its extensive influence, and the great quantity of it which is diffused through nature : for there are many reasons for being assured, that the 69th degree below o is far from being the extremity of cold, or lowest possible degree of heat. When you shall be acquainted with the effects of mixture, and with the nature of the materials employed in these mixtures, you will not find it difficult to form a judgment with respect to this matter.

2. *Of the Distribution of Heat.*

A second improvement in our knowledge of heat, which has been attained by the use of thermometers, is the more distinct notion we have now than formerly, of the *Distribution* of heat among different bodies.

I remarked formerly, that, even without the help of thermometers, we can perceive a tendency of heat to diffuse itself from any hotter body to the cooler around, until it be distributed among them, in such a manner that none of them are disposed to take any more heat from the rest. The heat is thus brought into a state of equilibrium. This equilibrium is somewhat curious. We find that when all mutual action is ended, a thermometer, applied to any one of the bodies, acquires the same degree of expansion : therefore the temperature of them all is the same, and the equilibrium is universal. No previous acquaintance with the peculiar relation of

each to heat could have assured us of this, and we owe the discovery entirely to the thermometer. We must therefore adopt, as one of the most general laws of heat, that " all bodies " communicating freely with each other, and exposed to no " inequality of external action, acquire the same temperature, as " indicated by a thermometer." All acquire the temperature of the surrounding medium.

By the use of these instruments we have learned, that if we take 1000, or more, different kinds of matter, such as metals, stones, salts, woods, cork, feathers, wool, water and a variety of other fluids, although they be all at first of different heats, let them be placed together in the same room without a fire, and into which the sun does not shine, the heat will be communicated from the hotter of these bodies to the colder, during some hours perhaps, or the course of a day, at the end of which time, if we apply a thermometer to them all in succession, it will point precisely to the same degree. The heat, therefore, distributes itself upon this occasion, until none of these bodies has a greater demand or attraction for heat than every other of them has ; in consequence of which, when we apply a thermometer to them all in succession after the first to which it is applied has reduced the instrument to its own temperature, none of the rest are disposed to increase or diminish the quantity of heat which that first one left in it. This is what has been commonly called an equal heat, or the equality of heat among different bodies ; I call it the *equilibrium of heat*. The nature of this equilibrium was not well understood, until I pointed out a method of investigating it, Dr. Boerhaave imagined, that when it obtains, there is an equal quantity of heat in every equal measure of space, however filled up with different bodies ; and professor Muschenbroeck expresses his opinion to the same purpose: " Est " enim ignis æqualiter per omnia, non admodum magna, dis- " tributus, ita ut in pede cubico auri et aëris et plumarum, par " ignis sit quantitas." The reason they give for this opinion is, that to whichever of those bodies the thermometer be applied, it points to the same degree.

* Muschenb. Phisica.

But this is taking a very hasty view of the subject. It is confounding the quantity of heat in different bodies with its general strength or intensity, though it is plain that these are two different things, and should always be distinguished, when we are thinking of the distribution of heat. If, for example, we have one pound of water in one vessel, and two pounds in another, and these two quantities of water are equally hot, as examined by the thermometer, it is evident, that the two pounds must contain twice the *quantity* of heat that is contained in one pound. But, undoubtedly, we can suppose that a cubical inch of iron may contain more heat than a cubical inch of wood, heated to the same degree ; and we cannot avoid being convinced of this by daily experience. Let the iron and wood, for example, be heated in a bread-oven, to the greatest degree which the oven commonly gives, and let them then be taken up and held in the hands of the person who lifts them ; it is certain that the iron will feel hotter, and will continue for a longer time to feel hot in the hand which holds it, than the wood will do ; that is to say, the iron will communicate more heat to the hand, before it assume the temperature of the hand, than the wood will do. Or, on the contrary, let the iron and wood be both covered with snow, until the snow has reduced them to its own temperature, and then let them be both taken up, and held in the hands ; the iron, in this case, will feel colder than the wood, and will continue for a longer time to feel cold in the hand which holds it ; that is, it will draw heat faster, and will continue for a longer time to draw heat from the hands, before it assume the temperature of that hand. It is therefore plain, that the iron has a greater capacity and attraction for heat, or the matter of heat, that the wood has ; and that, when both of them have been exposed a due time to the same heating cause, and appear equally hot by the thermometer, it contains a greater quantity of heat, or of the matter of heat, than the wood does ; and, therefore, in cooling them both down to the temperature of the hand, a greater quantity of heat comes out of the iron. Or, on the contrary, if both are equally cold, as examined by the thermometer, and we then make them both warm to the same degree, the iron takes in much more heat than the wood.

It was formerly a common supposition, that the quantities of heat required to increase the heat of different bodies by the same number of degrees, were directly in proportion to the quantity of matter in each; and therefore, when the bodies were of equal size, the quantities of heat were in proportion to their density. But very soon after I began to think on this subject, (anno 1760) I perceived that this opinion was a mistake, and that the quantities of heat which different kinds of matter must receive, to reduce them to an equilibrium with one another, or to raise their temperature by an equal number of degrees, are not in proportion to the quantity of matter in each, but in proportions widely different from this, and for which no general principle or reason can yet be assigned. It will be proper to consult, on this subject, the Comment. de Rebus in Medicina Gestis, vol. 21, and vol. 26, containing the valuable experiments of Jo. Carl. Wilcke, extracted from the Swedish Transactions. Also experiments of Professor Godolin, in the Nova Acta Reg. Societ. Upsalensis, tom. 5. This opinion was first suggested to me by an experiment described by Dr. Boerhaave (Boerhaave Elementa Chemiæ, exp. 20, cor. 11.) After relating the experiment which Fahrenheit made at his desire, by mixing hot and cold water, he also tells us, that Fahrenheit agitated together quicksilver and water unequally heated. From the Doctor's account, it is quite plain, that quicksilver, though it has more than 13 times the density of water, produced less effect in heating or cooling water to which it was applied, than an equal measure of water would have produced. He says expressly, that the quicksilver, whether it was applied hot to cold water, or cold to hot water, never produced more effect in heating or cooling an equal measure of the water than would have been produced by water equally hot or cold with the quicksilver, and only two-thirds of its bulk. He adds, that it was necessary to take three measures of quicksilver to two of water, in order to produce the same middle temperature that is produced by mixing equal measures of hot and cold water.

To make this plainer by an example in numbers, let us suppose the water to be at the 100th degree of heat, and that an equal measure of warm quicksilver at the 150th degree, is suddenly mixed and agitated with it. We know that the middle temperature between 100 and 150 ; is 125, and we know that this middle temperature would be produced by mixing the cold water at 100 with an equal measure of warm water at 150; the heat of the warm water, being lowered by 25 degrees, while that of the cold is raised just as much. But when warm quicksilver is used in place of warm water, the temperature of the mixture turns out 120 degrees only, instead of 125. The quicksilver, therefore, is become ·less warm by 30 degrees, while the water has become warmer by 20 degrees, only ; and yet the quantity of heat which the water has gained is the very same quantity which the quicksilver has lost. This shews that the same quantity of the matter of heat has more effect in heating quicksilver than in heating an equal measure of water, and therefore that a smaller *quantity* of it is sufficient for increasing the sensible heat of quicksilver by the same number of degrees. The same thing appears, whatever way we vary the experiment ; for, if the water is the warmer mass and quicksilver the less warm one, by the above difference, the temperature produced is 130. The water, in this case, is become less warm by 20 degrees, while the heat it has lost, being given to the quicksilver, has made this warmer by 30 degrees. And lastly, if we take three measures of quicksilver to two of water, it is no matter which of them be the hotter. The temperature produced is always the middle temperature between the two, or 125 degrees, in the temperatures already mentioned. Here it is manifest that the same quantity of the matter of heat which makes *two* measures of water warmer by 25 degrees, is sufficient for making *three* measures of quicksilver warmer by the same number of degrees. Quicksilver, therefore, has less *capacity* for the matter of heat than water (if I may be allowed to use this expression) has ; it requires a smaller quantity of it to raise its temperature by the same number of degrees.

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The inference which Dr. Boerhaave drew from this experiment is very surprising. Observing that heat is not distributed among different bodies in proportion to the quantity of matter in each, he concludes that it is distributed in proportion to the space occupied by each body; a conclusion contradicted by this very experiment. Yet Muschenbroeck has followed him in this opinion.

As soon as I understood this experiment in the manner I have now explained it, I found a remarkable agreement between it and some experiments made by Dr. Martin (Essay on the Heating and Cooling of Bodies) which appeared at first very surprising and unaccountable; but, being compared with this one, may be explained by the same principle. Dr. Martin placed before a good fire, and at an equal distance from it, a quantity of water, and an equal bulk or measure of quicksilver, each of them contained in equal and similar glass vessels, and each having a delicate thermometer immersed into it. He then carefully observed the progress, or celerity, with which each of these fluids was heated by the fire, and raised the thermometers. He found, by repeated experiments, that the quicksilver was warmed by the fire much faster than the water, almost twice as fast; and after each experiment, having heated these two fluids to the same degree, he placed them in a stream of cold air, and found that the quicksilver was always cooled much faster than the water. Before these experiments were made, it was supposed that the quicksilver would require to heat or cool it a longer time than an equal bulk of water, in the proportion of 13 or 14 to one.

But, from the view I have given of Fahrenheit's, or Boerhaave's experiment with quicksilver and water, the above of Dr. Martin's is easily explained. We need only to suppose that the matter of heat, communicated by the fire, was communicated equally to the quicksilver and to the water, but that, as less of it was required for heating the quicksilver, than for heating the water, the quicksilver necessarily was warmed fastest of the two. And when both, being equally heated, were exposed to the cold air to cool, the air at first took their heat from them equally fast,

but the quicksilver, by loosing the same quantity of the matter of heat that the water lost, was necessarily cooled to a greater degree ; it therefore became cold much faster than the water. These experiments of Dr. Martin, therefore, agreeing so well with Fahrenheit's experiment, plainly shew that quicksilver, notwithstanding its great density and weight, requires less heat to heat it, than that which is necessary to heat, by the same number of degrees, an equal measure of equally cold water. The quicksilver, therefore, may be said to have less capacity for the matter of heat. And we are thus taught, that, in cases in which we may have occasion to investigate the capacity of different bodies for heat, we can learn it only by making experiments. Some have accordingly been made, both by myself and others. Dr. Crawford has made a great number of very curious ones, and his Theory of the Heat of Animals is founded partly on some experiments made in this manner, the result of which is given in his book on that subject.

It appears, therefore, from the general result of such experiments, that if we had a thousand masses of matter, of the same size and form, but of different materials, and were to place them all in the same room, until they assumed the same temperature ; were we then to introduce into that room a great mass of red hot iron, the heat of which, when communicated with all these different bodies at the same time, might be sufficient for raising the temperature of them all, by 20 degrees ; the heat thus communicated from the iron, although it produced an equal effect on each of these bodies, in raising its temperature by 20 degrees, would not however be equally divided or distributed among them. Some of them would attract and retain a much greater quantity of this heat, or matter of heat, than others ; and the quantity received by each would not be in proportion to their densities, but in proportions totally unconnected with it ; and perhaps not any two of them would receive precisely the same quantity, but each, according to its particular capacity, or its particular force of attraction for this matter, would attract and require its own peculiar quantity to raise its temperature by the 20

degrees, or to reduce it to an equilibrium or equality of
saturation with the surrounding bodies. We must, there-
fore, conclude that different bodies, although they be of
the same size, or even of the same weight, when they are
reduced to the same temperature or degree of heat, what-
ever that be, may contain very different quantities of the
matter of heat; which different quantities are necessary
to bring them to this level, or equilibrium, with one
another.

It may be here remarked, that the discoveries which
have been made in this way are very unfavorable to one
of the opinions which have been formed of the nature of
heat. Many have supposed that heat is a tremulous, or
other, motion of the particles of matter, which tremulous
motion they imagined to be communicated from one body
to another. But, if this were true, we must admit that
the communication would be in conformity with our gene-
ral experience of the communication of tremulous motion.
We are not at liberty to feign laws of motion different from
those already admitted, otherwise we can make any suppo-
sition account for any phenomena that we please. The den-
ser substances ought surely to be the most powerful in
communicating heat to others, or exciting it in them. The
fact, however, in a great many examples, and yet not in all,
is just the reverse. Such an opinion is therefore totally
inconsistent with the phenomna. I do not see how this
objection can be evaded.

When a sufficient number of experiments shall have been
made to ascertain with exactness the capacity of differ-
ent bodies for the matter of heat, we may try a new method,
different from that of Sir Isaac Newton, for estimating the
violent degrees of heat, which are far above the reach of
the thermometer. Suppose, for example, that a lump of
stone is taken red hot from a strong fire, and plunged into
water, and that, after a little while, when it and the water
are reduced to the same temperature, the water is found
warmer than before, by 10 degrees. Suppose also, that we
have learned by other experiments, that, when heat is com-
municated from this stone to an equal bulk of water, for
every one degree by which the water becomes warmer, the

stone becomes less warm by two degrees.   Then, if the stone and water in this experiment were equal in bulk, it is plain that, while the water was heated by 10 degrees, the stone must have been cooled 20 degrees; but if, instead of their being equal in bulk, the stone had only the fiftieth part of the bulk of the water, it must have been cooled 50 times as much, or was 1000 degrees hotter before it was plunged into the water than it is now, for otherwise it could not have communicated 10 degrees of heat to fifty times its bulk of water.

We have now sufficiently explained what has been learn-ed of the distribution of heat among different bodies, the knowledge of which subject, so far as we have yet advanc-ed in it, has been acquired by the use of thermometers. *(See Note 1. at the end of the volume.)*

3. *Of the Celerity of the Communication of Heat.*

A third improvement which has been made in our know-ledge of heat by the use of thermometers, is in the inves-tigation of the celerity with which it is commucated from hotter bodies to colder ones, in different cases; and of the progression by which its transmission is regulated on those occasons.   A number of experiments relative to these questions are collected and compared in some of Dr. Mar-tin's essays; and, in my opinion, a general account of what may be inferred from them may be given in three propositions, expressing general laws of the communication of heat, and by explaining a sort of deceptions which oc-cur in making the experiments.

The first of these propositions is, that for any thing we yet know to the contrary, heat, or the matter of heat, is disposed to enter in to, or to leave the different kinds of matter with nearly equal celerity; or, that among the dif-ferent kinds of matter, there is no difference, so far as we know, in the celerity with which the matter of heat is dis-posed to enter into them, or leave them.

When the experiments which have been made to enquire into this point are exaimned, they seem at first to lead to a very different conclusion.   Some kinds of matter, in the same circumstances, are found to be heated and cooled

much faster than others. The first opinion formed upon this
subject was, that the times necessary for the heating or
cooling of equal masses of different matter are directly in
proportion to the density of the masses; or that the cele-
rity with which bodies of equal size are heated or cooled,
is inversely as their density, or directly as their rarity.
Lord Verulam first entertained this opinion, or rather of-
fered it as a suspicion: but Dr. Boerhaave afterwards
adopted it, and was so much persuaded of it, that he gives
a list or a number of substances differing greatly in densi-
ty, such as gold, quicksilver, water, oil, spirit of wine,
æther, air, &c. and he affirms, that if equal bulks of these
different bodies be reduced to the same temperature, and
then exposed to the action of the same heating cause, capa-
ble of making them hotter by 100 degrees, for example,
each will require a different length of time from the rest
to receive that temperature; the denser bodies requiring a
longer time in porportion to their density.

But this opinion is entirely the work of imagination,
without any foundation in fact; and, when we have re-
course to experiments, the result proves it to be widely
erroneous. This appears from some experiments of Dr.
Martin's, one of which is the experiment on the heating
and cooling of quicksilver and water, already described:
in which the quicksilver, instead of requiring a much
longer time than water to its being heated or cooled, was
heated or cooled in the same circumstances, much faster
than the water.

I am persuaded that when different kinds of matter
are equally exposed to the same heating cause, the mat-
ter of heat enters into them all at first with nearly equal
celerity; at least we have no facts to prove the contrary.
But there is necessarily a deception in the appearances,
some of them becoming hot sooner, and some much
slower than others, and for this reason, that those which
have the greatest capacity for the matter of heat, are not
so soon saturated with it as others. They require a
greater quantity of it, and a longer time for that quantity
to be communicated to them; and I apprehend that the
time necessary for the heating or cooling of different

bodies of the same size, and in the same circumstances and conditions, will be found nearly proportional to the capacity which each body has for the matter of heat, or to the quantity of it necessary to raise its temperature by a given number of degrees; and that, in Dr. Martin's experiment with quicksilver and water, although the quicksilver became hot, and appeared to take in the heat faster than the water, it did not in reality take it in faster, but, having less capacity for heat, was sooner saturated to that degree which accorded with the circumstances in which it was placed.

For any thing, therefore, that we yet know to the contrary, there is no difference among the different kinds of matter, with respect to the celerity with which heat is disposed to enter into them, or leave them, when the circumstances are exactly the same. (*See Note 2. at the end of the volume.*)

The second proposition, or general law, which may be deduced from the experiments on the heating and cooling of bodies is that the celerity with which heat is communicated from hotter bodies to colder ones, is, when other conditions are equal, nearly proportional to the difference of their temperature, or to the intensity of the heat in the hot body above that of the cold.

Of this general fact we have examples in the experiments that have been made to learn the progress with which bodies cool, when exposed to a stream of cold air. The heat is taken away fastest at first, and more and more slowly afterwards, in proportion as the temperature of the mass approaches nearly to that of the air which is moving against it.

Sir Isaac Newton supposed, that, in this case, the quantities of heat lost, in equal and small portions of the time, are exactly proportional to the excess of heat remaining in the hot body at the beginning of such small portions, or to the difference between its temperature and that of the air; and, therefore, that the quantities of heat lost in equal divisions of time are a series of proportionals, or in a geometrical progression[*].   This idea of Sir Isaac Newton's is supported or verified by some experiments which

* Phil. Trans. abridged, IV. 2. p. 3.

were made, in a great variety of circumstances, by the
ingenious Professor Richman, and published in the
transactions of the Petersburgh academy for the year
1750.

Dr. Martin, it is true, also made some experiments
relative to this question, by which experiments he thought
it appeared that the progression of cooling varied a
little from this rule*.   But I am not satisfied with Dr.
Martin's experiments on this subject.   I think they were
attended with a circumstance that might deceive him.
There is room, however, for more inquiry into this point
by further experiments.

It is proper, also, to remark, that experiments made
with this view can have no value, except when the heat is
taken from the mass, or communicated to it, by a stream
of air playing constantly and uniformly on it, and all the
time at the same temperature; for in this case only can it
act in a regular manner.   If, on the contrary, the air be
not in motion, but in a stagnating state; and still more, if
the hot body be inclosed with a small quantity of it, the
natural progress of cooling must be totally deranged, as in
this case, the cooling cause will not continue the same,
the air becoming warmer every moment, while it cools
the hot body, and therefore less fit to take away the rest
of its heat.   This enables us to understand why, in cold
weather, if the air is calm, we are not so much chilled as
when there is wind along with the cold; for in calm wea-
ther, our clothes, and the air entangled in them, receive
heat from our bodies ; this heat is accumulated in them
to a certain degree, and brings them nearer than the
surrounding air to the temperature of our skin.   But
in windy weather, this heat is prevented, in a great
measure, from accumulating ; the cold air, by its im-
pulse, and rapid succession, both cooling our clothes fas-
ter, and carrying away the warm air that was entangled in
them.

The sensation of coldness, therefore, produced by
wind, or agitated air, is so much stronger than that pro-
duced by equally cold air in a stagnating state, that we
are often persuaded the agitated air is actually colder,

* Martin's Essays, p. 55, &c.

until we examine it by the thermometer ; and Dr. Boer-
haave thought the deception so strong, that he contrived
an experiment to remove it completely (Boerhaave Ele-
menta Chemiæ). He suspended a thermometer in the
air of a large room, for some time, and noteing the
degree to which it pointed, he then directed against the
bulb of it a stream of air, impelled by a large bellows, in
the same room. That stream of air would certainly feel
to a person who opposed any part of his body to it, con-
siderably colder than the rest of the air in the same room ;
but the thermometer is not in the least affected by it.
And it would be easy to exhibit another experiment, to
shew, that agitated air is not made colder by agitation.
A piece of ice, for example, being suspended in the air
of a warm room, and blown upon by a bellows, instead
of being thereby kept the more cool, as our hand
would be, and preserved the longer from being totally
melted, would certainly be melted so much the faster,
than when the air is allowed to stagnate in some measure
around it.

The last of the three propositions which I said may be
deduced from the experiments on the communication of
heat is, that

The celerity with which heat is communicated from
hotter bodies to colder ones, when all other conditions
are equal, is proportionable to the extent of contact,
and closeness of communication, between the bodies.

Every person is prepared to admit this general rule
by common experience. We all understand that, if
we had two balls, and two cubes of gold, and one of
each was made very hot in an oven, and then laid upon
the other, the heat would be communicated much faster
from the hot cube to the cold one, than from the hot
ball to the cold one, which is touched by it in one point
only.

Of the general fact expressed by this proposition, we
have examples in the experiments that have been made
on the heating and cooling of masses of the same matter,
but of different forms, and different sizes, exposed at the
same time to be heated by a constant and equal heating

cause, or to be cooled by a stream of cold air. It being at the surface that the body has contact and communication, either with the cold air or with the heating cause, the extent of contact and communication of parts is, in this case, the whole surface. Accordingly, those forms which contain the same quantity of matter under the greater surface, are the more quickly heated or cooled, in proportion to the extent of that surface ; an ounce of gold in the form of a cube, is heated and cooled sooner than the same quantity in the form of a ball ; and if it be beaten into the form of a thin plate, it is heated and cooled incomparably sooner, in proportion as the surface is then the more extended ; and, for the same reason, when the masses we compare have the same form, and differ only in their size, the smaller are heated or cooled faster than the larger, or in the shorter time, in the direct proportion of their dimensions. If we have, for example, two cubes of gold, in one of which the diameter is only the half of that of the other, and both of them are plunged into boiling water to be heated ; or after they are heated, to be exposed to a stream of cold air to cool, the smaller will be heated or cooled in half the time that is required by the other; for, in the smaller body, the surface bears twice the proportion to the quantity of matter, and consequently to the quantity of heat, which is to be either communicated or taken away, that it does in the larger, the quantity of matter being as the cube, and the surface only as the square of the diameter. And, accordingly, some experiments made by Sir Isaac Newton, and by Dr. Martin, agree sufficiently with this general rule, which applies equally to all masses of unequal size, but of the same matter, and having the same form, whatever that be.

This enables us to understand how it happens, that in those arts in which much heat and fuel are employed, as in the extraction of metals from their ores, foundaries, distilleries, and the like, experience has shewn, that the larger they make their furnaces and stills, and other such utensils, they do their work with so much the less fuel ; for, in the larger furnaces and stills, and other such utensils, the extent of surface, communicating with the air,

bears a smaller proportion to the contents of the furnace or still, and to the quantity of fuel employed, or of heat produced, than in smaller utensils of the same nature ; and the waste of heat is therefore less.

We can also explain, by the same general principle, the slowness with which heat is communicated from one part to another of those bodies, or assemblages of bodies, which are uncommonly rare or spongy, through which it passes much more slowly than through the dense and heavy. A thick rod of iron, for example, or copper, if one end of it be put into the fire, and made red hot, transmits the heat so quickly, and in such quantity, to the other end, that it cannot be touched without burning the hand ; while a stick of wood of the same size can easily be held by the one end, while the other is burning. A piece of cork transmits heat through it still more slowly than the wood, or is a worse conductor of heat ; and a mass of wool, or feathers, or furs, excel even the cork in the slowness with which heat is conducted through them. Such materials compose a mass so spongy and rare, and between the parts of which there is so little closeness of contact, that the heat is communicated with difficulty through them ; and, although the interstices or pores of such bodies are filled with air, this being a fluid excessively rare, and which, in this case, is kept entangled, and is in a stagnating state, it gives little assistance in the transmission of heat. These are the reasons why such materials are so effectual in keeping our bodies warm in very cold weather. In very cold countries, the inhabitants are not only wrapped in a thick clothing of furs by day, but cover themselves with feather beds by night. Frequent experience of the effect of these materials disposes us to consider them as warm, or as having the power of adding heat to the bodies wrapped in them ; but this is a mistake, they only preserve the heat of such bodies from being dissipated so fast as it would otherwise be ; and, by the same quality, they may be, and are, employed to keep things cool in some cases. If, for example, we wish to preserve a lump of ice from melting fast in a warm room, we cannot do better than to wrap it in plenty of flan-

nel, or furs, or the like materials, which produce the desired effect, by retarding the communication of heat to the ice from the air, and other bodies contained in the room.

These coverings, therefore, of wool, furs, feathers, and such like materials, keep us warm, not by adding heat to our bodies, but by preserving our natural heat from being dissipated too fast.

In the same manner does snow preserve the ground from being deep frozen, and the vegetables from being destroyed, in the very cold countries. Nothing can be imagined more spongy, soft, and downy, than snow that has not been compressed; nothing is more fit to serve this purpose. The showers of it that fall in this climate, may be considered as accidental, and of little importance; the moderate temperature of our winters rarely requires such a preservative. But in those parts of the world that are exposed to violent cold, it is well known to fall regularly in the beginning of winter, until it cover the ground to the depth of several feet, and, where the cold is excessive, to that of several yards, and even of fathoms. Thus nature, which has clothed the animals of those regions with the richest and warmest furs, throws over the ground this thick and downy covering, which has a wonderful effect in preserving its heat. It cannot, it is true, preserve it at a higher degree than the freezing point; for unless the surface be cooled to this degree, the snow cannot be on it. But this degree is a great degree of heat, when compared with the terrible and deadly cold which is common during the winter in the air of those climates. In countries exposed to severe winters, the thermometer commonly falls to the beginning of Fahrenheit's scale, or 32 of his degrees below frost: a degree of cold which is just as much colder than simple frost, as frost is colder than the warmth of our summer weather. And, in the countries the most remarkable for severe winters, a cold has often been observed, which was more than twice as far below frost. In Siberia, at Krasnojark, east of Siberia, and in some of our settlements in Hudson's Bay, the thermometer has often fallen to 35 and 45 degrees below o of Fahrenheit: a degree of cold in which

quicksilver was congealed by simply exposing it to the cold air.

In such dreadful colds, a great depth of snow lying on the ground has much effect in preserving the vegetables.

It is proper, however, in this place to remark, that this effect of the rarity, or sponginess of bodies, this retardation of communication, or transition of heat through them, does not take place in fluids equally as in solids. Fluids generlly re-ceive heat, and transmit it through them more qucikly, than the greater number of solids. Of this we have an obvious example in the air, which, though excessively rare and light, cools the bodies exposed to it remarkably fast; much faster than we might expect from our knowledge of its rarity.

This depends, partly, on the capacity which such fluids may have for the matter of heat, but more on their great ex-pansibility by heat, and on the easy mobility of their parts among one another; which two last qualities, combined in the same matter, produce a particular effect, which occa-sions the quick dispersion of heat through it. This effect is a constant motion, or change of the fluid in contact with the heated body.

If, for example, we expose a hot body, as a lump of red hot iron, to the air to cool, the air in contact with it becomes immediately hot, while it takes heat from the iron. In be-coming hot it is also greatly expanded, and rendered more light than the surrounding parts of the atmosphere. It is, therefore, pressed upwards by these, in the same manner as a piece of Cork, a quantity of oil, or any lighter substance, is pres-sed upwards when we attempt to sink it in water. It does not, therefore, remain in contact with the iron, but is forced to rise in a stream, and leave its place to colder air, which comes from all around. This again is soon heated and expanded, and made to rise in the same manner, and thus fresh and cool air is ap-plied to the hot iron in constant succession, by which its heat is carried off much faster than if the matter in contact with it were perfectly stagnant.

This ascending motion of the air that is in contact with a hot body becomes evident when we hold our hand perpendicularly

over it, though at a considerable distance, or when we place the hot body in the light of the sun, and so that the shadow of it may be thrown on a white wall : a waving stream, like a stream of smoke rising from the shadow, is seen on the wall. This appearance is caused by the rarefied air, which, having its power to refract or attract light considerably changed by its rarefaction, the light which passes through it is turned out of its course to one side or the other, in a fluctuating manner, and therefore does not fully and regularly illuminate that part of the wall to which it was tending, but produces an imperfect and unsteady illumination, which resembles the shadow of smoke.

Another effect produced by this motion of warm air, is the tremulous appearance of objects seen at a distance through the smoke, or heated air, arising from a fire, or from chimney tops, or from dry ground much heated by the sun; for the light which comes from these objects to the eye, is disturbed a little from its straight course, by passing through the streams of warm air ; and coming, therefore, to the eye, with successive changes of its direction, occasions an unsteadiness in the apparent place of the object, or parts of the object, from which it comes.

We also learn from this effect of heat upon air, the reason of the common opinion, that heat, or fire, has a tendency upwards. This opinion has been founded on the stream of warm air which may be felt ascending from any hot body exposed to the atmosphere ; and the appearance and properties of flame have also contributed to the support of such an opinion. Flame, in consequence of its being composed of a luminous fluid, rarer than air, is driven upwards, in this last, on every occasion. To be convinced, however, that heat has not a prevalent tendency upwards, we need only shut up a hot body in a glass receiver, and exhaust the air: we shall then find the heat penetrating equally through every part of the receiver; or, if it affects some more than others, it is those that are nearest the hot body. But, let the air be admitted into the receiver, the uppermost part of the glass will soon be the hottest, although the most distant from the hot body.

Heat, therefore, by rarefying air, and thereby producing a motion in the parts of this fluid, is more quickly and equally dispersed through it than through any solid quantity of matter, and occasions the appearances above described.

There is one other fact that depends on this principle, and which deserves particular notice, on account of its importance. This fact is the ventilation of mining works that are properly planned, and the proper effect of fire-places and vents in houses rightly constructed.

In mines that are properly planned, a stream of air passes constantly through the mine, and with this remarkable circumstance, that it has one direction in summer, and another in winter.

To understand this, it is necessary to know, that in beginning to mine for metals or minerals, the mountain or rising ground in which they are contained, is first pierced in an horizontal direction, that the water which abounds in subterraneous places may run out constantly, while the workmen proceed forwards to the vein of ore. This horizontal entrance into the mountain, or mining ground, is called the level, drain, or drift of the mine, or sometimes the adit. But when they have proceeded forward a certain length, they find themselves distressed for want of fresh air. The long excavation they have made contains a quantity of air in a stagnating, or nearly stagnating state, which is exhausted of its vital principle by their breathing it, and by their candles. Their candles, therefore, first burn dim, and at last are extinguished, and give them warning that their own lives are in imminent danger. The best remedy for this is, to dig a wide pit perpendicularly from the surface over the internal extremity of the level, so deep as to communicate with it. No sooner is this done, than there is plenty of fresh air in the mine, a current is immediately established, which in summer enters through the pit, and passes out through the level, and in winter as invariably enters through the level, and rises up through the pit.

The reason of this is, that the heats of summer, and the colds of winter do not penetrate so quickly into the interior

parts of the earth, as to produce there the same changes of temperature that happen in the atmosphere. The communication of heat through large masses of solid matter being slow, there is not sufficient time, during the summer, for the heat of the air, or that produced by the sun's light, to act so completely on the mass of this globe, or to give to the internal parts of it the summer heat of the atmosphere; and, for the same reason, the winter is too short to admit of such a loss of heat from the interior parts of the earth, as to bring down the heat of these to a level with that of the surface and the atmosphere.

In the interior parts, therefore, the course of time has produced a temperature, between the heat of summer, and the cold of winter, which undergoes very little variation. Of this we have experience when we place thermometers in deep caves, or pits, that have little communication with the atmospherical air, or when we plunge them into the water of perennial springs, where they rise out of the ground. The thermometer points to the same degree in summer and in winter; which degree is the medium between the two seasons in that particular part of the earth's surface. It is evident, therefore, that the internal parts of the earth, and along with them, the air contained in the cavities of mines, must be warmer in winter, and colder in summer than the external atmosphere. Hence the column of air contained in the pit of the mine is heavier in summer, and lighter in winter, than an equal column of the external air; and this occasions the motion which takes place,....for the internal column gravitating on the internal extremity of the level, is counterpoised by an external column which gravitates on the external extremity of the same level, and, whenever one exceeds the other in density and weight, the heavier must force the lighter one to ascend. The internal column, therefore, descends in summer, and is forced upward in winter, its place being supplied by external air, which enters during the first season at the top of the pit, and during the latter at the external extremity of the level. And this air, when it enters, being very soon reduced to the temperature of the internal parts of the earth, the same

conditions continue to take place, and the current of air through the mine continues as long as the heat of the external air is different from the temperature of the internal parts of the earth. The truth of this explication is remarkably confirmed, by a stagnation of the air in such mines, which now and then happens in the spring and autumn, the atmosphere in those seasons sometimes assuming the middle temperament. The cause of the motion is thus discontinued, and the workmen must have, at such times, recourse to other means, to obtain a renewal of the air in the mine ; such as, kindling a fire at the bottom of the pit, to give a little superiority of heat to the column of air which it contains, or directing into it the force of the wind to set it in motion. But these expedients are employed only on particular occasions. The ventilation of mines, in general, depends on the principle above explained. If it should appear difficult to imagine how the external air should thus pass constantly through the mine, without reducing it to a degree of heat coinciding with its own, it must be considered that the solid mass of matter, through which the mine is dug, is so enormously great, when compared with the small stream of air which passes through the works, that this air cannot have any sensible effect in changing the temperature of the internal parts of the mine. They, no doubt, receive a little heat in this way during the summer, and lose a small part of their heat during the winter ; but this acquisition or loss is too small to prevent the ordinary conditions from continuing in force. It remains only to be further observed, with regard to this subject ; that many mines are so extensive, or of such a nature, that it is necessary for drawing out the water or the rubbish, or other materials cut out of the mine, to dig several pits, at considerable distances from one another, to communicate with different parts of the subterraneous excavations. These different pits, however, produce the same effect as the single pit and level, above supposed ; for being different from one another in depth, in consequence of the unevenness of the earth's surface, the pits which are dug in the higher parts of the ground contain an higher column of air than those dug in the lower ; and, though the temperature

of heat is the same in them all, and equally different from
that of the atmosphere, the difference of height produces a
preponderancy in the one or the other; and the same effect
is produced in ventilating the mine. The same principles
explain the ventilation, or draught of air, as it is called
in common chimneys and vents,....and the vents of the
lower rooms in a house carry up the smoke better than
the vents of the upper rooms that are nearer the top of
the house; but the venting of chimneys is liable to many
accidents and variations, which are afterwards to be con-
sidered.

All these motions produced by heat in atmospherical air,
and the quick dispersion of it through it, depends on two
qualities, formerly mentioned as belonging to fluids in general;
I mean the easy mobility of their parts among one another,
and their expansibility by heat; and as these two qualities
are found in air, in an eminent degree, it is, therefore, the
more remarkably affected in the manner I have describ-
ed.

All other fluids, however, are affected in a similar manner,
by heat applied to them; but not being so expansible, the
motion produced in them is not so conspicuous. But a mo-
tion is produced in them, as well as in air, which occasions
the heat applied to be more quickly and equally dispersed
through them than through most solid bodies.

When water, for example, is set in a vessel over the fire,
the heat is necessarily communicated first to the lower parts
of the fluid, and, while these receive the heat, they are ex-
panded and become lighter than the rest of the water; they
are therefore driven upwards, and their place at the bottom is
supplied by the colder, heavier parts of the fluid. These,
however, are soon heated, and made to rise in their turn,
and thus there is a constant motion, or circulation of the parts
of the water, which occasions a quick and equal distribution
of the heat through it. For this reason, water is often em-
ployed in chemistry, as a medium through which heat is com-
municated to vessels of glass, these being most secure from
being broken when the heat is applied to them gradually

and equally, over the whole of their surface. When water is applied to this purpose in chemistry, it is called the *Water Bath.*

A motion, depending on the same principle, is also the cause which hinders the water of deep lakes from being frozen during the winter, in this climate, and at times when the surface of pools, or other shallow collections of water, is totally frozen. The cold air, while it passes over the surface of the water, necessarily cools it. The superficial water, by being thus cooled, becomes denser and heavier than the water that is below it. It therefore sinks downwards, in innumerable small streams, leaving its place at the surface to be occupied by the less cold water which rises from below. This, in its turn, is deprived of a part of its heat, and disposed to sink in the same manner; and thus a constant circulation of the parts of the water is established, a constant exchange between the surface and the bottom; in consequence of which there is such a quantity of water to be cooled, when the lake is large and deep, that there is not sufficient time, during our frosty season, for its being all cooled to near the freezing point. And it must be cooled to within a few degrees of that point, before that part which is at the surface will remain there long enough to be frozen*.

But large seas, which contain collections of water incomparably greater than those of lakes, and which are agitated by winds and currents, are, in consequence of these circumstances, so little affected by the cold of winter, that their temperature continues nearly the same through the whole year, like that of the interior parts of the earth. And this mild temperature of the ocean affects the air which passes over it, and the countries that are entirely or mostly surrounded with it; when places surrounded with extent of continent, or exposed to winds blowing over a northerly continent, are affected in winter with violent cold. As the heat of summer penetrates but slowly into the solid mass of this globe, so, during the winter, it comes out again but slowly.

* When water is cooled to 6 or 8 degrees above freezing, it is not made sensibly denser by a diminution of heat....*De Luc.*

The solid parts of the globe are at rest among one another, and cannot be set in motion by moderate heat, like the parts of fluids. Heat, therefore, is transmitted through them with that slow progress with which it commonly pervades large masses of solid matter. The cold winds, therefore, while they blow over a great continent, very soon cool the surface of the earth. The heat of the interior parts of it does not quickly ascend to temper that coldness, as it does from the bottom of large collections of water. The surface, therefore, is cooled to a great degree, and the cold winds afterwards, while they pass over this surface, not meeting with any thing that can diminish their coldness, act with their full force on the matter exposed to them. *(See Note 3. at the end of the volume.)*

When we now reflect on those motions which heat produces in fluids, the circulation of them which it occasions, and the quick and equal distribution of heat through them, which follows from thence, we find one phenomenon in nature, which, on the first view of it, may appear inconsistent with these general principles. This phenomenon is the unequal distribution of heat in the atmosphere, in which it is well known that the lower parts are much warmer than the superior. We have satisfactory proofs of the coldness of the superior parts of the atmosphere, when we ascend high mountains, and other elevated parts of the earth's surface, in whatever climate be their situation. The French academicians who were sent to measure a degree of the meridian at the equator, when they left the sultry plains of South America, and ascended the high mountains of that part of the world, passed through a succession of strata of the atmosphere, increasing in coldness, and distinctly marked by their productions, as well as by the manner in which they affected their feelings, and the thermometer, until they reached the regions of perpetual frost and the vast collections of snow, which are but little wasted there by the melting, but continue to accumulate annually, until they overhang the precipices of the mountains, and fall at last with a dreadful ruin on the country that is below them.

The same perpetual frost, and accumulation of snows, with the consequences of it, are well known to take place on all other high mountains, such as the Alps and Pyrenees; and the coldness of the superior parts of the atmosphere has been further experienced by those who have ascended from the plains to a great height, by the use of baloons. To all those proofs, we may add the showers of hail which sometimes fall in the heat of summer and the piercing cold with which these are accompanied, in consequence of there mixing a part of the superior with the inferior air.

It is, therefore, plain that only the lower region of the air is warm, the heat being found to diminish while we ascend in the atmosphere, and that the very high regions of it are ex-cessively cold. This has long been known, and attempts have been made to explain how it happens.

The explanation that has been commonly given is, that the light of the sun reflected from the sides of the mountains, and other elevated parts, into the valleys and plains, is the cause of the greater heat of the air in these. But it is easy to shew that this opinion is a great mistake. If the declivities of the mountains, and other elevated parts of the earth, had polished surfaces, the light would be reflected in the manner supposed. But the surface of the earth is every where com-posed of rough uneven materials, which absorb the greater part of the light that falls on them, and disperse the rest in every possible direction; the consequence of which is the contrary to what is here supposed; for the mountains must receive more reflected or dispersed light from the plains than that which the plains can receive from the mountains. The quantity of light dispersed from the surface of mountains and plains, is in general proportional to the extent of those surfaces; and plains are generally far more extensive than those declivities of mountains, from which they can receive dispersed light; and, moreover, the greatest number of ex-tensive plains are at such a distance from mountains, that it cannot be supposed that they owe their superior heat to light reflected from these. There are parts of them, from which the mountains are at such a distance that they cannot be seen,

and yet these parts are as warm, and generally warmer, than those which are in the neighborhood of the mountains.

The principles from which a good explication of this phenomonon may be deduced, are two well known qualaties of the atmosphere,....its transparency, and its elasticity or compressibility. A consequence of the transparency of the atmosphere is, that the light of the sun, in passing through it to the surface of the earth, does not communicate heat to it, or communicates only a very small quantity, especially in the superior regions, which are much more clear and transparent than the inferior. It is the nature of light to excite heat, chiefly in opaque bodies, which stop its progress and absorb it; but very little in those that are transparent, and allow it to pass through them. To be satisfied of this, with respect to air, we need only to reflect on our own experience. We have all felt, that in a clear day in the spring season, or in the beginning of summer, the air is often very cool, although the sun shines bright, and has great power to warm the solid and opaque bodies which are exposed to rays. Stones, slates plates of metal, and other dry and opaque bodies, will become warmer than the human body, though the air at the same time feels remarkably cold. We have another strong proof of this, when we attend to the focus of a large burning glass, or mirror, exposed to the sun. Although any solid or opaque body, introduced into the spot, is melted or dissipated, or consumed in an instant,....before it is put there, we cannot, by looking at the place, perceive any signs of that intense and terrible heat by which it is so quickly affected. The truth is, that there is no great heat in that spot, until the opaque body is placed in it. There is a great quantity of light passing through that place, but the air being transparent, little heat is produced. There is another experiment, by which the effect of transparency, in a medium transmitting light, is further demonstrated. If a large lens be exposed to the sun, and a tub of clear water be placed below it, so situated that the focus of the lens shall be formed below the surface of the water, and in the centre of its mass, there are no remarkable signs of great

heat produced in that focus; but let a stick be thurst down, and introduced into that spot, the water will soon boil on the surface of the stick, and the internal parts of the wood will be scorched to a black coal, by the intense light which penetrates into them, and acts so powerfully upon the surface.

It is therefore evident that the sun warms the atmosphere, by first warming the surface of the earth, from which the heat is communicated to the air immediately above it; and, no doubt, it must act also on the vapors and effluvia which diminish the transparency of the lower region of the atmosphere. But its greatest action is on the earth. We can perceive this in clear sunshine, and when the sun is high, by looking at distant objects over the ridge of a rising ground. Those objects, or their parts, appear to have a tremulous motion, the light which comes from them to the eye being disturbed in its course by the rising of warm air, which is constantly receiving heat from the surface of the soil.

Thus we have accounted for the heat being found chiefly in the inferior region of the atmosphere: we must next explain why it remains in a great measure there, and is not so quickly communicated to the superior regions as we might expect, when we consider the expansibility of the air, and the motions which heat applied to it commonly produces.

This partial distribution and confinement, in some measure, of heat, to the lower region of the atmosphere, depends on the elasticity and compressibility of the air. A consequence of this quality is, that the lower region of it is condensed by the pressure of the superior parts; and, although it be expanded by the heat in it, and thus made rarer than it would otherwise be, this expansion is not equal to the condensation it suffers by the pressure of the superior parts*. The lower air, therefore, although heated and expanded, continues still denser than the strata of the atmosphere that are considerably above it, and is not disposed to be pressed or driven far

* When we descend from high mountains, the air is found warmer by one degree for every 200 feet of perpendicular descent: but the pressure of 200 feet perpendicular of the atmosphere, has two and a-half times as much effect in condensing the air, as one degree of heat has in rarefying it.

upwards in them, but remains in its place near the surface of the earth, where the heat is most useful for the production and support of vegetables and animals*. Thus we are enabled to understand the excellent effects of a proper degree of shelter in increasing the fertility of a country, and the comfort of its inhabitants. Whatever preserves the lower regions of the air from frequent and violent agitation, improves the climate in our northern latitudes, by preventing more or less the chilling effects of wind, and dispersion of the warmth accumulated at the surface of the earth. This shelter is procured, to a certain degree, by a due proportion of plantations of timber trees, properly disposed over the face of a country, so as to break or diminish the force of the winds.

If it should be here asked, what is meant by the lower region of the air, and what is the limit between it and higher regions? the answer must be, that there is no precise limit; but that, in general, by the lower region, I mean the parts of the atmosphere which do not reach to an elevation equal to that of high mountains. Very few of the highest mountains rise to the height of two miles above the surface of the sea; but we know for certain, by the appearance of some meteors, as well as by calculation, that our atmosphere exists, and is capable of action at the height of fifty miles, though at that height, by the want of pressure, it is in a highly rarefied state.

Even the lower region, as above pointed out, is very different in different parts of it; those parts nearest the earth, and in the lowest situations, being in general the warmest, as well as the most compressed; while the heat of those above them, and the pressure to which they are exposed, gradually diminishes.

The diminution of the heat is about one degree of Fahrenheit for every two hundred feet of elevation, and the multi-

* The celebrated Dr. Franklin explained, in much the same manner, the unequal distribution of heat in the atmosphere; and deduced from these principles the phenomena of whirlwinds, hurricanes, and water-spouts, which never before were so ingeniously and satisfactorily accounted for. See Phil. Trans. vol. 55,....and Experiments and Observations on Electricity, &c by Benjamin Franklin, p. 182 and 216.

plication of bulk is proportional (very nearly) to the elevation of temperature. The greatest difference of heat, therefore, balances two-fifths of the effect of compression, and cannot, alone, raise the air contiguous to the earth's surface to any considerable height. Were it not for the winds, the region of perpetual snow, even under the equator, would not be a mile from the level of the sea. Thus, there is a gradual and undefinable transition from the warmth of the lower air to the coldness of the higher; and, in the higher regions, the same gradual diminution of heat and compression takes place.

This view of our atmosphere enables us to understand why those horizontal motions of the air, which we call winds, and which have some effect in mixing the superior colder with the inferior warmer air, produce this effect in a far less degree than might be apprehended, although it often happens that they have different directions in the different strata; of the atmosphere at the same time; for the strata, which differ greatly in their heat, may easily move horizontally in different directions, without mixing together, being separated by a multitude of intermediate strata, in which there is a regular and slow gradation of density and heat. We were led to give the preceding views of the communication, distribution, and progression of heat, as examples of some of the improvements we have made in our knowledge of heat, since the invention of thermometers : and this invention arose from the discovery of the expanding power of heat.

Having, therefore, now done with the consideration of expansion, we proceed to consider the next of the general effects of heat, formerly enumerated, which is fluidity.

SECT. II....OF FLUIDITY.

THE propriety of considering fluidity as one of the general effects of heat, is sufficiently obvious. It is well known that a great number of different substances, which commonly appear in a solid state, can be rendered fluid by heat, and will continue, so long as they continue heated to the proper degree. But, *besides this consideration*, we find reason to be persuaded, that fluidity, wherever we meet with it in the various kinds of matter, is *always* the effect or consequence of heat; experience, in most cases, having taught us, that those bodies which commonly appear in a fluid state, and the fluidity of which might be thought a natural and essential quality, do nevertheless derive this fluidity from the quantity of heat they contain; for, when we diminish their heat sufficiently, they do not fail to assume a solid form ; from which it is plain that the fluid state in which we commonly see them depends upon heat ; nor is there any other circumstance, so far as we can observe, that is necessary and indispensable to their fluidity.

In general, therefore, we have the strongest reasons for considering heat as the efficient cause of fluidity, in all bodies in which this quality is found.

But, as there is a great diversity among different bodies, with respect to the intensity of heat necessary to produce this effect on them, this shews that there are certain peculiarities in their constitution, which increase in some, and diminish in others, the disposition to fluidity ; and these peculiarities are plainly certain differences in the nature, number, and composition of their constituent parts ; a proof of which is, that we can by mixture, in most cases, change a body from its ordinary state, in this respect. Water, for example, by having salt or spirit of wine mixed with it, acquires an increase of its disposition to fluidity, and is enabled to endure, without freezing, a more intense cold than it can bear in its ordinary state. But still its fluidity, however much it may resist the cold, depends

upon heat; for, by continuing to take heat from it, it will freeze at last. Moreover, as long as it does not undergo any new change with respect to mixture, no other cause but a certain quantity of heat alone can render and preserve it fluid.

Our experience of the freezing or consolidation of fluids, when exposed to more or less powerful degrees of cold, is almost universal. The exceptions are very few. The strongest spirit of wine, and a few subtile and volatile oils, are the only substances which have not yet been congealed by any degree of cold hitherto known. As these, however, are so few in number, it appears unreasonable to believe that they have a nature and constitution so different from that of all other bodies, that fluidity is in them an essential quality, of which they cannot be deprived by any diminution of their heat. As we have no certain knowledge of what is the lowest possible degree of heat, or most extreme intensity of cold, but, on the contrary, shall have reason hereafter to be persuaded that the most violent cold which has yet been observed, is very far short of the most extreme degree, it is more reasonable to suppose that these few bodies differ from others, only in having a much greater *disposition* to fluidity, and therefore as requiring less heat to preserve them fluid; in consequence of which, we have never yet known a degree of cold sufficient for congealing them; but that they would undoubtedly congeal, like other fluids, were they exposed to a sufficient cold.

Quicksilver was, not long since, one of this small number of bodies, which, having never been seen in any other than a fluid state, was considered as naturally and essentially fluid, and incapable of being reduced alone to a solid form, until experiments were made with it, first in different parts of the Russian empire, since the year 1760, and verified afterwards in other places. By these experiments, every person must be convinced that quicksilver is a metal, which can become solid and malleable like the rest, but that it is melted with so little heat, that the lowest degree ever observed, over the

greater part of the surface of this earth, is more than sufficient to preserve it fluid.

In the same manner may we consider all other fluids as solids melted by heat.

Some philosophers, however, have offered many objections against admitting this general proposition concerning the nature of fluids. They thought it necessary to suppose that water is an exception. They could not be persuaded that in it fluidity is the effect of heat, but supposed this quality to be an essential quality of the water, depending on the spherical form and polished surface of its particles, and that the freezing of it depends on the introduction of some extraneous matter. This opinion is defended by Professor Muschenbroeck, in his physics*, and he has collected all the reasons and arguments which have been devised for supporting such an opinion.

But when we consider these reasons and arguments with due attention, we do not find that any of them are valid.

Many of them are alleged facts, adduced to prove that water is frozen on some occasions, and in some circumstances, under which Mr. Muschenbroeck did not comprehend how it should be cooled to the degree reckoned necessary to its congelation ; and he therefore concludes that the freezing of it must proceed from some other cause than the diminution of its heat to the proper degree.

But had he applied a good thermometer to it, he would have found that it was actually cooled to the usual degree in every case in which it was frozen. For the truth of this we may depend on the testimony of Dr. Martin, who, with the assistance of his friends, took care to have experiments made, in many distant parts of the world, and at different times, with thermometers, on the goodness of which he could entirely rely. There is no doubt that many mistakes have been committed by using bad ones, or by the want of skill to . use them properly. But the truth is, that in the facts adduced by Muschenbroeck, no thermometer whatever was applied to the water itself, but only vague reasoning is employed, to make

* Muschenb. Phys. de aqua.

it appear probable that it was not cooled to the proper degree.
We may, therefore, pass over the greater number of his rea-
sons, and take some notice of a few only of his facts, which
are surprising in themselves, and could not be explained by
any principles then known, and which besides are stated by
him in such a manner as to make them appear uncommonly
perplexing.

The fact which first induced him to think that ice is formed
by the entrance of extraneous matter into water, is the en-
largement of bulk ; ice being always more bulky than the
water from which it was produced, in the proportion of about
9 to 8. This enlargement of bulk is the effect of a powerful
cause acting with great force, which has been shewn by
many experiments. " Now he thinks it more probable that
" this enlargement of bulk, and the force with which it
" is performed, depend on the entrance of some subtile
" matter, than on the simple diminution of heat, the gen-
" eral effect of which is to occasion contraction in bodies."

To the whole of this argument, however, it is easy to an-
swer, that the fact is better explained by the supposition of a
particular modification of the attraction, which makes the parts
of the water unite and cohere, in one way rather than in any
other.

The attraction of cohesion, which, in this case, is the at-
traction of the particles of the water for one another, is un-
doubtedly the cause of the concretion of water into ice, as it
is the cause of the solidity and hardness of all other bodies.
No person doubts that the hardness of iron is owing to the
cohesive attraction of its particles for one another. We can
make these particles change their position among one ano-
ther, by hammering the iron, or drawing it into wire, and
they still cohere. And yet, when iron that has been melted
is allowed, by cooling, to resume its solidity, which it does
when it is yet in a strong red heat, the same effect is produc-
ed as in the case of water ; the iron, when solid being a little
more bulky than when it was fluid. And the reason is the
same in both cases,....the attraction of cohesion, which
was counteracted and overcome in these bodies, by the re-

pulsive power of heat; being, in consequence of the diminu-
tion of the heat, allowed to operate, it acts in a peculiar man-
ner, forcing the particles to assume an arrangement different
from that which they had while the body was fluid, and which
requires a little more space. This peculiar action of the at-
traction of cohesion in such bodies, I formerly compared to
that of a great number of small magnets set afloat on the sur-
face of water. There is no doubt that by the attraction of
their north and south poles, they would arrange themselves
in a particular manner, different from that of the closest ar-
rangement; and that something like this does actually hap-
pen in freezing water, and congealing iron, is sufficiently
evident from a number of phenomena formerly described.

Another remark of Professor Muschenbroeck is, that " in
" moderate frosty weather, water, exposed and preserved
" from disturbance, remains fluid, but being then disturbed, it
" suddenly freezes." To this it may be answered, that there
is here no appearance which gives reason for concluding that
the freezing of it depends on the introduction of extraneous
matter. And when we consider the experiment more fully,
which is to be done in the sequel, we shall find that it can be
better explained, in a very different manner.

3dly, " A wet cloth hung in the air, in cold and dry weather,
" will sometimes become stiff, by the freezing of the humid-
" ity in it, when the thermometer shews the cold of the air to
" be insufficient for the freezing of water in ordinary circum-
" stances." And Mr. Muschenbroeck supposed, " that, in
" this case, the ice was formed in the cloth by some other
" cause than the diminution of heat, and the power of attrac-
" tion." But he formed this judgment from the indication
of the thermometer hung in the air. Had he applied it to the
cloth itself, he would have found it cooled to the usual degree
of 32 Fahrenheit; and the truth is, that a wet cloth, hung
up in these circumstances, becomes colder than any other
body around it; which fact shall be explained hereafter.

4thly, " Water, at 33 degrees of Fahrenheit, when the
" strong nitrous acid is added to it, becomes warmer to the
" 41st degree. But, if we pour the same strong acid on

" pounded ice, at 32 degrees of Fahrenheit, an enormous cold
" is the consequence. How can these different effects be
" produced, when the heat of the water and ice are so
" nearly the same, unless there be in the ice some hetero-
" geneous matter, the power of which is increased by the
" acid?"

To this we can answer, that ice is certainly different from
water, and produces different effects, when mixed with some
other substances ; and the question is, In what does this dif-
ference consist?   Mr. Muschenbroeck says that it consists
in the presence of frigorific particles in the ice ; but we
shall soon give another account of it, that shall explain this
experiment, without having recourse to such a supposition.

5thly, He concludes, " That in ice the particles of water
" have corpuscles of a different kind mixed with them ; for
" this reason that the water produced by the melting of ice,
" is unfit for boiling vegetables tender, or for drawing a good
" infusion from tea, coffee, &c. until it has been boiled for
" some time."

In this reason, it is stated as a fact, that the water of melted
ice is different from good and soft water, but that it becomes
soft and good by boiling it some time.   Now this fact appears
exceedingly improbable, and before it be admitted, would
need to be ascertained by accurate and decisive experiments ;
for, in whatever manner ice may be different from water, that
difference should certainly be removed when the ice is chang-
ed again into water.   The Professor does not mention any
accurate experiments made by himself or others, to prove
this alleged fact, and some I have made disprove it entirely.
I am persuaded that the opinion of its truth is vulgar and er-
roneous, and has arisen from the coldness of the water which
comes from ice ; for it is well known, that very cold water
applied to soap, has little power to dissolve it, or appears to
be hard water, when compared with the same water less
cold ; and it is a maxim in cookery, that water employed for
the boiling of vegetables does not perform its office well, when
the vegetables are put into it in its cold state, although it had
not been frozen for a long time before.   It may therefore be

presumed, that the vulgar opinion of the unfitness of ice-water to dissolve soap, or boil vegetables well, or to draw good infusions from vegatable substances, has arisen from its being applied to them colder than other water.

6thly, He states as fact, " That when a mixture is made of " snow and common salt in a proper vessel, and in the middle " of this mixture another is placed, containing pure water; if " we now set the whole apparatus upon a fire, while the salt is " dissolved, and the snow melts, the pure water is frozen, and " the fire accelerates the freezing of the pure water ; for the " stronger the fire is, the pure water is frozen so much the " sooner, which (he adds) could not happen, if the fire did " not chase the congealing particles from the snow into the " water ; and he cannot believe that any person will assert, " that in this case, the fire chases an absence or deficiency of " heat from the snow into the water."

It must be confessed, that this experiment, thus stated, appears very perplexing ; but this proceeds from the introduction of a circumstance into it, which is no way necessary to its success, and is introduced for no other reason but to make it appear more surprising. The circumstance I mean, is setting the whole apparatus upon a fire. The plain truth is, that the salt and snow act on one another in a way hereafter to be explained ; and, in consequence of this action, they unite together, and are both liquefied, becoming at the same time intensely colder than they were before. This will be sure to happen, and with more effect, though we do not set the apparatus upon a fire. But the power of this mixture, when made in proper quantity, is so great, that although the heat received from the fire diminishes the cold produced, there is still enough for freezing the water, which, when frozen, is always as cold as in ordinary cases. There is only one case in which the application of heat can promote or quicken the action of the mixture, and this happens when the snow and salt, before they are mixed, are both as cold as the Zero in Fahrenheit's scale, or are near to this cold. When so cold as this, they are little disposed to act upon one another; and if heat be applied to them, it quickens their action. But even in this case, the heat applied diminishes their power, upon the whole, to pro-

duce cold ; for, if time be allowed, they will be sufficient for
congealing a larger quantity of water, without the application
of heat to them, than when it is applied.   And this experi-
ment is to be explained in the sequel, without employing the
supposition of congealing or frigorific particles.

Upon the whole, there are many of Professor Muschen-
broeck's reasons for his opinion on the congelation of water,
that are quite inconclusive.   And though some must appear,
for the present, unaccountable facts, these are hereafter to be
explained.   But, in the mean time, it is evident that none of
them give any satisfactory proof that the fluidity of water is
an essential quality, or that any new matter is introduced into
it when it is frozen.   The propensity which many have to
imagine water is a substance naturally and essentially fluid, is
a prejudice contracted from the habit of seeing it often every
day in a fluid state, or much oftener in this state than in the
state of a solid.

As we have sufficient reason, therefore, for beleiving that flui-
dity is in all cases the effect of heat, and not an essential quantity
of any of the palpable kinds of matter, so it appears to be one of
those effects which are produced generally, and upon all kinds
of bodies.   There are very few substances now known, which
cannot, in some degree of heat or other, be brought into a
state of fluidity by heat.   Those that are the least disposed to
it, are the earths and stones, many of which were formerly
reckoned incapable of being melted ; but later experiments
have shewn this opinion to have been erroneous, especially
some experiments with burning glasses, and some made in
consequence of improvements in the construction of furnaces,
and in the management of fuel, by which means much more
violent heats have been produced than formerly, and which
have brought into perfect fusion many of those bodies which
were reckoned incapable of being melted by heat.

In considering this effect of heat, we may first remark one ob-
vious difference between it and expansion, which is, that where-
as expansion is, in general, an equable and regular effect, accom-
panying every increase of the heat of bodies, the change of a bo-
dy from the state of solidity, to that of fluidity does not take

place, except when its heat is encreased, so as to rise above a par-
ticular degree. This has been ascertained by innumerable expe-
riments made with thermometers. If the heat of the body be
increased above this degree, it becomes fluid in all the higher
degrees. If again its heat be diminished, so as to fall below
this degree, it becomes solid, and remains solid in all the
lower degrees. This at least may by stated as the general fact.

There are, however, many substances in which the transi-
tion is not so sudden. These, in a certain latitude of heat,
are reduced to an intermediate state, a state of softness, and
pass through all the degrees of it, while they are changing
from a solid to a fluid, by heat. We have examples of this
in bees wax, resin, tallow, glass, and many other substances.
But, even in these, every degree of softness depends on a cor-
responding degree of heat, which has the power to produce
it ; and, in most of these bodies, there is also a remarkable
step from the greatest degree of softness to perfect fluidity,
which always depends on a certain intensity of heat neces-
sary to the perfect liquefaction of that particular body.

We may therefore say in general, that each different kind
of matter requires to be heated to a particular degree, or above
it, to render it fluid, and that below this degree it is always
solid, or has some solidity. This degree of heat is there-
fore called the CONGEALING or the MELTING POINT of such
body. It is also called the congealing point of such bodies
as appear commonly in a state of fluidity, and the melting
point of those that are solid, in ordinary circumstances. This
point, or intensity of heat, is invariably the same for each par-
ticular kind of matter, no other circumstance but heat appear-
ing to have the smallest influence in producing fluidity, so
long as the body itself continues the same, unaltered in the
mixture or porpotion of its constituent parts. But, when we
compare different kinds of matter, there is all the variety that
can be imagined between those that require for their fusion
the utmost violence of heat, and those for which so little is
sufficient, that they always appear, in ordinary circumstances,
in a fluid form.

Among this variety there are a number, the liquefaction of which by heat is distinguished by the term of VITRIFICA-TION.

This term is applied to the fusion of those earthy bodies which undergo a certain change of their appearance and texture, by being melted and allowed again to congeal. Whatever might be their appearance before, in their natural state, they assume, after fusion, a resemblance of glass, forming a hard brittle mass, which breaks with equal ease in every direction, and into fragments, the surfaces of which are smooth, polished, and undulated. When an earthy substance assumes this form after fusion, it is said to be VITRIFIED.

Thus we have taken a general view of the subject, according to the notions which were formerly had of it.

I must, however, now add, that this account of fluidity, as an effect of heat, is not complete and satisfactory, if it be understood in conformity with the common opinions which were entertained of it. It is inconsistent with many remarkable phenomena; and these phenomena, when attentively considered, shew that fluidity is produced by heat, in a very different manner from that which was commonly imagined; a manner, however, which, when understood, enables us to explain many particulars relating to heat or cold, which appeared in the former view of the subject, quite perplexing and unaccountable.

Fluidity was universally considered as produced by a small addition to the quantity of heat which a body contains, when it is once heated up to its melting point; and the return of such body to a solid state as depending on a very small diminution of the quantity of its heat, after it is cooled to the same degree; that a solid body, when it is changed into a fluid, receives no greater addition to the heat within it than what is measured by the elevation of temperature indicted after fusion by the thermometer; and that, when the melted body is again made to congeal, by a diminution of its heat, it suffers no greater loss of heat than what is indicated also by the simple application to it of the same instrument.

This was the universal opinion on this subject, so far as I know, when I began to read my lectures in the University of Glasgow, in the year 1757. But I soon found reason to object to it, as inconsistent with many remarkable facts, when attentively considered; and I endeavored to shew, that these facts are convincing proofs that fluidity is produced by heat in a very different manner.

I shall now describe the manner in which fluidity appeared to me to be produced by heat, and we shall then compare the former and my view of the subject with the phenomena.

The opinion I formed from attentive observation of the facts and phenomena, is as follows. When ice, for example, or any other solid substance, is changing into a fluid by heat, I am of opinion that it receives a much greater quantity of heat than what is perceptible in it immediately after by the thermometer. A great quantity of heat enters into it, on this occasion, without making it apparently warmer, when tried by that instrument. This heat, however, must be thrown into it, in order to give it the form of a fluid; and I affirm, that this great addition of heat is the principal, and most immediate cause of the fluidity induced.

And, on the other hand, when we deprive such a body of its fluidity again, by a diminution of its heat, a very great quantity of heat comes out of it, while it is assuming a solid form, the loss of which heat is not to be perceived by the common manner of using the thermometer. The apparent heat of the body, as measured by that instrument, is not diminished, or not in proportion to the loss of heat which the body actually gives out on this occasion; and it appears from a number of facts, that the state of solidity cannot be induced without the abstraction of this great quantity of heat. And this confirms the opinion, that this quantity of heat, absorbed, and, as it were, concealed in the composition of fluids, is the most necessary and immediate cause of their fluidity.

To perceive the foundation of this opinion, and the inconsistency of the former with many obvious facts, we must con-

sider, in the first place, the appearances observable in the melting of ice, and the freezing of water.

If we attend to the manner in which ice and snow melt; when exposed to the air of a warm room, or when a thaw succeeds to frost, we can easily perceive, that however cold they might be at the first, they are soon heated up to their melting point, or begin soon at their surface to be changed into water. And if the common opinion had been well founded, if the complete change of them into water required only the further addition of a very small quantity of heat, the mass, though of a considerable size, ought all to be melted in a very few minutes or seconds more, the heat continuing incessantly to be communicated from the air around. Were this really the case, the consequences of it would be dreadful in many cases ; for, even as things are at present, the melting of great quantities of snow and ice occasions violent torrents, and great inundations in the cold countries, or in the rivers that come from them. But, were the ice and snow to melt as suddenly as they must necessarily do, were the former opinion of the action of heat in melting them well founded, the torrents and inundations would be incomparably more irresistible and dreadful. They would tear up and sweep away every thing, and that so suddenly, that mankind should have great difficulty to escape from their ravages. This sudden liquefaction does not actually happen ; the masses of ice or snow melt with a very slow progress, and require a long time, especially if they be of a large size, such as are the collections of ice, and wreaths of snow, formed in some places during the winter. These, after they begin to melt, often require many weeks of warm weather, before they are totally dissolved into water. This remarkable slowness with which ice is melted, enables us to preserve it easily during the summer, in the structures called Ice-houses. It begins to melt in these, as soon as it is put into them ; but, as the building exposes only a small surface to the air, and has a very thick covering of thatch, and the access of the external air to the inside of it is prevented as much as possible, the heat penetrates the ice-house with a slow progress, and this, added to the slowness with which the

ice itself is *disposed* to melt, protracts the total liquefaction of it so long, that some of it remains to the end of summer. In the same manner does snow continue on many mountains during the whole summer, in a melting state, but melting so slowly, that the whole of that season is not a sufficient time for it complete liquefaction.

This remarkable slowness with which ice and snow melt, struck me as quite inconsistent with the common opinion of the modification of heat, in the liquefaction of bodies.

And this very phenomenon is partly the foundation of the opinion I have proposed; for if we examine what happens, we may perceive that a great quantity of heat enters the melting ice, to form the water into which it is changed, and that the length of time necessary for the collection of so much heat from the surrounding bodies, is the reason of the slowness with which the ice is liquefied. If any person entertain doubts of the entrance and absorption of heat in the melting ice, he needs only to touch it; he will instantly feel that it rapidly draws heat from his warm hand. He may also examine the bodies that surround it, or are in contact with it, all of which he will find deprived by it of a great part of their heat; or if he suspend it by a thread, in the air of a warm room, he may perceive with his hand, or by a thermometer, a stream of cold air descending constantly from the ice; for the air in contact is deprived of a part of its heat, and thereby condensed and made heavier than the warmer air of the rest of the room; it therefore falls downwards, and its place round the ice is immediately supplied by some of the warmer air; but this, in its turn, is soon deprived of some heat, and prepared to descend in like manner; and thus there is a constant flow of warm air from around, to the sides of the ice, and a descent of the same in a cold state, from the lower part of the mass, during which operation the ice must necessarily receive a great quantity of heat.

It is, therefore, evident, that the melting ice receives heat very fast, but the only effect of this heat is to change it into water, which is not in the least sensibly warmer than the ice was before. A thermometer, applied to the drops or small

streams of water, immediately as it comes from the melting
ice, will point to the same degree as when it is applied to the
ice itself, or if there is any difference, it is too small to deserve
notice. A great quantity, therefore, of the heat, or of the
matter of heat, which enters into the melting ice, produces no
other effect but to give it fluidity, without augmenting its sen-
sible heat ; it appears to be absorbed and concealed within the
water, so as not to be discoverable by the application of a ther-
mometer.

In order to understand this absorption of heat into the
melting ice, and concealment of it in the water, more distinct-
ly, I made the following experiments.

The plan of the first was, to take a mass of ice, and an
equal quantity of water, in separate vessels, of the same size
and shape, and as nearly as possible of the same heat, to sus-
pend them in the air of a warm room, and, by observing with
a thermometer the celerity with which the water is heated,
or receives heat, to learn the celerity with which it enters
the ice ; and the time necessary for melting the ice being
also attended to, to form an estimate, from these two data,
of the quantity of heat which enters the ice during its lique-
faction.

In order to prepare for this experiment, I chose two thin
globular glasses, four inches diameter, and very nearly of the
same weight : I poured into one of them five ounces of pure
water, and then set it in a mixture of snow and salt, that the
water might be frozen into a small mass of ice. As soon as
frozen, it was carried into a large empty hall, in which the
air was not disturbed nor varied in its temperature during the
progress of the experiment ; and in this room the glass was
supported, as it were, in mid air, by being set on a ring of
strong wire, which had a tail issuing from the side of it five
inches long, and the end of this tail was fixed in the most
projecting part of a reading desk or pulpit : And in this
situation the glass remained until the ice was completely
melted.

When the ice was thus placed, I set up the other globular
glass precisely in the same situation, and at the distance of

18 inches to one side, and into this I poured five ounces of water, previously cooled, as near to the coldness of melting ice as possible, viz. to 33 degrees, and suspended in it a very delicate thermometer, the bulb of which being in the centre of the water, and the tube being so placed, that without touching the thermometer, I could see the degree to which it pointed. I then began to observe the ascent of this thermometer, at proper intervals, in order to learn with what celerity the water received heat, stirring the water gently with the end of a feather about a minute before each observation. The heat of the air, examined at a little distance from the glasses, was 47 degrees of Fahrenheit's scale.

The thermometer assumed the temperature of the water in less than half a minute, after which, the rise of it was observed every five or ten minutes, during half an hour. At the end of that time, the water was grown warmer than at first, by seven degrees; and the temperature of it had risen to the 40th degree of Fahrenheit's scale.

The glass with the ice was, when first taken out of the freezing mixture, four or five degrees colder than melting snow, which I learned by applying the bulb of the thermometer to the bottom of it; but after some minutes, it had gained from the air those four or five degrees, and was just beginning to melt, which point of time was then noted, and the glass left undisturbed ten hours and a half. At the end of this time, I found only a very small and spongy mass of the ice remaining unmelted, in the centre of the upper surface of the water, but this also was totally melted in a few minutes more; and, introducing the bulb of the thermometer into the water, near the sides of the glass, I found the water there was warmed to the 40th degree of Fahrenheit. From this it appears, that when a considerable part of the ice was melted, and the quantity of water around it was increasing, the heat was not transmitted through this water to the remaining ice altogether so fast as it was received by the water; which is easily understood, if we consider that the distance between the remaining ice and the external surface of the vessel through which the heat entered, was gradually increasing,

and that heat always requires time to pass through bo-
dies, or to be communicated from one part of them to
another*.

It appears, therefore, from this experiment, that it was
necessary that the glass with the ice receive heat from the
air of the room during 21 half-hours, in order to melt the
ice into water, and to heat that water to the 40th degree
of Fahrenheit.   During all this time, it was receiving the
heat, or matter of heat, with the same celerity (very near-
ly) with which the water-glass received it, during the sin-
gle half-hour in the first part of the experiment.   For, as
the water received it with a celerity which was diminish-
ing gradually during that half-hour, in consequence of the
diminution of difference between its degrees of heat and
that of the air ; so the glass with the ice also received
heat with a diminishing celerity, which corresponded ex-
actly with that of the water-glass, only that the progres-
sion of this diminution was much more slow, and corres-
ponded to the whole time which the water surrounding the
ice required to become warmed to the 40th degree of
Fahrenheit.   The whole quantity of heat, therefore, re-
ceived by the ice-glass during the 21 half-hours, was 21
times the quantity received by the water-glass during the
single half-hour.   It was, therefore, a quantity of heat,
which, had it been added to liquid water, would have made
it warmer by $\overline{40\text{-}33} \times 21$, or 7×21, or 147 degrees.   No
part of this heat, however, appeared in the ice-water, ex-
cept 8 degrees ; the remaining 139 or 140 degrees had
been absorbed by the melting ice, and were concealed in
the water into which it was changed.

The communication of heat to the melting ice was easi-
ly perceived, during the whole time of its exposition, by
feeling the stream of cold air which descended from the
glass.

* Water, while its heat is increased from the 33d to the 40th degree of
Fahrenheit, suffers very little or no expansion from the heat entering it
(Vide De Luc, *Recherches*, &c. p. 418.)   The water, therefore, in this,
case, would not be made to move and circulate by the unequal heat of the
different parts of it, but would remain in a great measure at rest.

This experiment was an analysis of the manner in which ice is melted by the heat of the air in ordinary circumstances.

But another obvious method of melting ice occurred to me, in which it would be still more easy to perceive the absorption and concealment of heat, and this was by the action of warm water.

When hot and cold water are mixed together, the excess of heat contained in the hot water is equally distributed in an instant through the whole mixture, and raises the temperature of it according to the greatness of the excess of temperature, and the proportion which the hot water bore to the cold. If the quantities of hot and cold water are equal, the temperature is the middle degree between that of the hot and that of the cold. No part of the heat disappears on this occasion, so far as we can perceive, but the intensity of it only is diminished, by its being diffused through a larger quantity of matter. It was, therefore, obvious, that if a quantity of heat is absorbed, and disappears in the melting of ice, this would easily be perceived when the ice is melted with warm water.

To make this experiment, I first froze a quantity of water in the neck of a broken retort, in order to have a mass of ice of an oblong form.

At the same time I heated a quantity of water nearly equal in weight to the ice, in a very thin globular glass, the mouth of which was sufficiently wide to take in the piece of ice. The water was heated by a small spirit of wine lamp applied to the bottom of the glass ; it was also often stirred with the end of a feather, and a thermometer hung in it.

While the water was heating, the mass of ice was taken out of the mould in which it had been formed, and was exposed to the air of a temperate room, until it was perceived to be beginning to melt over the whole of its surface.

I then put a woollen glove on my left hand, and taking hold of the ice, I wiped it quite dry with a linen towel, laid it in the scale of a balance on a piece of flannel, and

hastily counterpoised it with sand in the opposite scale, that I might examine the weight of it afterwards ; and I immediately plunged it into the hot water, and extinguished the lamp at the same time. The lamp being small, the heat of the water had been increasing very slowly, and had almost ceased to increase; and being examined immediately before I put the ice into it, the temperature was found to be just 190 degrees. The ice was all melted in a few seconds, and produced a mixture, the temperature of which was 53 degrees.

The weight of the ice, when put into the hot water, was seven ounces three drachms and a-half. The weight of the glass, with the whole mixture in it, was sixteen ounces, seven drachms, and seven grains. The weight of the glass alone was nearly one ounce.

In considering this experiment, we may overlook quantities less than half a drachm, or thirty grains, and reckon the quantities of the different articles, by the number of half-drachms in each.

Thus the weight of the ice was 119 half-drachms.
————— Hot water   135   _ _ ~   ᴗ
————— Mixture   254
————— Glass alone   16

The melting of the ice was affected, not only by the heat of the hot water, but also by that of the glass. And, by other experiments, I learned that sixteen parts of hot glass have no more power in heating cold bodies, than eight parts of equally hot water ; we may therefore substitute, in place of the sixteen half-drachms of warm glass, eight half-drachms of warm water, which, added to the above quantity of warm water, make up 143 half-drachms.

The heat of this warm water was 190 degrees, that is 158 hotter than the ice; and if this heat had abated in the mixture only in consequence of the quantity and coldness of the ice, and if nothing had happened when the ice was put in, but merely a communication of this heat, and an equal distribution of it through the mixture, the temperature of the mixture should have been 158, viz. the excess of heat in the warm water, multiplied by 143, the

quantity of the warm matter, and divided by 262, the quantity of the whole, which gives 86.

The mixture should have been 86 degrees warmer than melting ice ; but it was found only 21 degrees warmer. Therefore a quantity of heat has disappeared, which, if it had remained in a sensible state, would have made the whole mixture and glass warmer by 65 degrees than they were actually found to be. But this quantity of heat would be sufficient for increasing, by 143 degrees, the heat of a quantity of water, equal in weight to the ice alone. It was, however, absorbed by the ice, without in the least increasing its sensible heat*.

The result of this experiment coincides sufficiently with that of the former ; the difference is not greater than what may be expected in similar experiments, and might arise from the accident of the central parts of the mass of ice being colder than the surface, by one or two degrees.

I have, in the same manner, put a lump of ice into an equal quantity of water, heated to the temperature 176, and the result was, that the fluid was no hotter than water just ready to freeze.   Nay, if a little sea salt be added to the water, and it be heated only to 166 or 170, we shall produce a fluid sensibly colder than the ice was in the beginning, which has appeared a curious and puzzling thing to those unacquainted with the general fact.

It is, therefore, proved that the phenomena which attend the melting of ice in different circumstances, are inconsistent with the common opinion which was established upon this subject, and that they support the one which I have proposed.

But some persons may, perhaps, imagine that the heat which thus disappears does not truly enter into the melting ice, or become combined with that into which it is changed.   This heat is, perhaps, entirely extinguished and destroyed.   As heat has been supposed by some to

* These two experiments, and the reasoning which accompanies them, were read by me in the Philosophical Club, or Society of Professors and others in the University of Glasgow, in the year 1762, and have been described and explained in my lectures, there, and in Edinburgh, every year since.

consist in a rapid motion or tremor of the particles of bo-
dies, or of some subtile matter that is intermixed with
them, those who choose to adopt this opinion may imagine
that this motion may meet with friction and resistance in
the ice, and that a part of it may thus be destroyed, or the
moving parts brought to rest*.

This supposition, again, as well as the common opi-
nion, will be found inconsistent with many phenomena
which attend the freezing of water, while these phenomena
strongly support the idea I have proposed.

If, for example, we expose some pure water to the cold
air, in a sharp frost, and set beside it an equal quantity of
equally warm water, with a little salt or spirit of wine
added to it, to prevent it from freezing ; we may perceive,
by putting a delicate thermometer into each of these waters,
that they are both cooled by the air at first with the same
celerity, until they are brought down to the freezing
point, at the same, or very nearly the same moment of
time. After this, the progress of the salted water in
cooling will be continued without interruption, until it be
as cold as the air to which it is exposed, which we may
suppose to be ten degrees colder than freezing water.
But while this goes on, if we examine what happens in the
pure water, when it has been cooled down to the freezing
point, we shall not find in it the same uninterrupted pro-
gress of cooling : nor will it suddenly be changed into
ice, which it ought to be, according to the common opi-
nion. It will begin to be changed into ice, but with a very
slow progress, which will require a long time to com-
plete it; and, during all this time, it will continue at the
32d degree of Fahrenheit's scale, or 10 degrees warmer
than the air to which it is exposed, until it be totally

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* Indeed this supposition seems inseparable from this mechanical theory
of the formal cause of heat. It is amusing to observe how the mechanical
explainers of the chemical phenomena produce whatever effects they have
occasion for, by their invisible motions and actions. Here heat is lost by
friction and resistance, &c. The heat is produced, in many chemical solu-
tions, by the same frictions and violent knockings of the conflicting particles
in the act of solution....yet the same act of solution is accompanied with
intense cold, when salt and snow are mixed.

<div align="right">EDITOR.</div>

frozen ; after which, it will again begin to become colder, and proceed without interruption, until it be as cold as the surrounding air and the salted water.

But, from the beginning of its congelation to the completion of it, although it does not become sensibly colder, it is constantly communicating heat to the surrounding air. A warmer body cannot be in contact with a colder one, without communicating heat to it. We may easily satisfy ourselves, that the water, while congealing, is continually imparting heat to the surrounding air ; for, if we suspend a delicate thermometer immediately above the water, we shall find the instrument affected by an ascending stream of air, less cold than the air around. While the water is congealing, therefore, it is constantly imparting heat to the air, without becoming colder itself, and this heat must be that which had been absorbed and concealed in it, the last time it became water, since now, in losing its fluidity again, this heat comes out of it, although it did not appear immediately before in a sensible state*.

In the above described common process of freezing water, the extrication and emergence of the latent heat, if I may be allowed to use these terms, is performed by such minute steps, or rather with such a smooth progress, that many may find difficulty in apprehending it ; but I shall now mention another example, in which this extrication of the concealed heat becomes manifest and striking.

This example is an experiment, first made by Fahrenheit, but since repeated and confirmed by many others.

He wished to freeze water from which the air had been carefully extracted. This water was contained in small glass globes, about one-third filled and accurately closed, to prevent the return of the air in to them. These globes were exposed to the air in frosty weather, and remained so

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* Dr. Black made this experiment also, and many registers of it are to be found among his memorandums : but, in our temperate climate, it is seldom that a freezing cold continues with sufficient steadiness for admitting a simple calculation. He made many experiments on the congelation of other substances, in more usual and steady temperatures : but, in this elementary course, intended for the instruction of the most uninformed hearer, he chose to confine his proofs to the most familiar observations.

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long exposed, that he had reason to be satisfied that they
were cooled down to the degree of the air, which was six
or seven degrees below the freezing point.   The water,
however, still remained fluid, so long as the glasses were
left undisturbed, but, on being taken up and shaken a little,
a sudden congelation was instantly seen.

It has since been found, by the trials of others, that the
experiment will succeed, although the water be not de-
prived of its air, and that the circumstances the most es-
sentially necessary are, that it be contained in vessels of a
small size, and preserved carefully from the least disturb-
ance.   The vessels, therefore, ought to be covered with
paper, or otherwise, to prevent slight motions of the air
from affecting the surface of the water*.   In these cir-
cumstances, it may be cooled to six, or seven, or eight
degrees below the freezing point, without being frozen ;
but, if it be then disturbed, there is a sudden congelation,
not of the whole, but of a small part only, which is formed
into feathers of ice, traversing the water, in every direction,
and forming a spongy contexture of ice, which contains
the water in its vacuities, so as to give to the whole the
appearance of being frozen.   But the most remarkable
fact is, that while this happens, (and it happens in a moment
of time) this mixture of ice and water suddenly becomes
warmer, and makes a thermometer, immersed in it, rise to
the freezing point.

Nothing can be more inconsistent with the old opinion
concerning the cause of congelation than the phenomena
of this experiment.   It shews that the loss of a little more
heat, after the water is cooled down to the freezing point,
is not the most necessary and inseparable cause of its con-
gelation, since the water is cooled 6, 7, or 8 degrees below
that point, without being congealed.

Some may, perhaps, imagine that the water remains
fluid in consequence only of some state of arrangement
and connection between its particles, which is favorable
or necessary to its fluidity, and that this state of arrange-
ment and connection continues in this over-cooled water,

* *Marian sur la Glace*

in consequence of its being carefully preserved from every disturbance.

Although this be only a supposition, let us admit it for a moment, and try how it will agree with the facts.

If this supposition were true, then, by disturbing such over-cooled water, we should suddenly bring on a total congelation of it, and it should continue as cold as it was immediately before ; but neither the one nor the other of these events takes place. A small portion only of the water is thus suddenly frozen, and it does not continue equally cold ; a quantity of heat suddenly appears in it, which was not perceptible immediately before : And this heat, being diffused in a moment through the whole mixture of water and newly formed ice, raises the temperature so high, that no more of the water can be frozen, until more of its heat be taken away.

But these phenomena, though inconsistent with such a supposition, and with the old opinion of the manner in which heat produces fluidity, strongly support the one which I have proposed, and can all be explained by it without difficulty. They actually shew the extrication from the freezing water of that quantity of heat which is concealed in its composition so long as it retains the form of a fluid. This experiment shews, that when water is cooled in a state of perfect rest, in a small vessel, it is disposed to retain this concealed heat, which I have been used to call it's latent heat, a little more strongly than in ordinary circumstances. In common circumstances, the water retains the whole of this heat, until it be cooled to the 32d degree of Fahrenheit, or a very little lower. If, in ordinary circumstances, we attempt to make it colder, we may perhaps succeed in making it still colder by one degree or two, but no more, for then the latent heat begins to be extricated from a small part of the water, and to appear in the form of sensible heat, that small portion of the water which loses it, assuming consequently the form of ice. Thus, the latent heat of the rest of the water continues afterwards to be extricated slowly and imperceptibly, in proportion as we abstract the sensible heat into which it is changed, until the whole of the water is frozen, or has lost the whole of its latent heat.

But, when the water is cooled in the circumstances of the above mentioned experiment, it retains its latent heat and fluidity longer, or until its sensible heat be diminished to seven or eight degrees below frost ; and, when it is in this state, if it be agitated, or suddenly disturbed by the impulse of the air, or the falling into it of a little bit of ice, or other such matter, this occasions the extrication of a part of the latent heat, which now becomes sensible heat, and that part of the water which thus loses its latent heat is at the same time changed into ice. But the heat thus extricated at once being in greater quantity than what is extricated in any one moment in the common process of congelation, it is more conspicuous, by suddenly increasing very remarkably the sensible heat of the materials, and limiting the quantity of the ice that is thus suddenly formed.

This surprising experiment, therefore, which formerly appeared so strange and unaccountable, can thus be explained. When this experiment is made with great care, in a vessel inaccessible to the external air, and in a place where it is not disturbed by the tremulous motion occasioned by walking on the floor, or the rattling of heavy carriages, it is possible to cool the water even ten degrees below 32°. If the water be now touched, ever so gently, with a slender spicula of ice, formed by crystallization, or a flake of dry snow, it instantly shoots into beautiful spiculæ, which rapidly form and branch out in all directions, and the thermometer, which had been left in it, rises slowly to the 32d degree.

But farther, by the same principles I also explained some other remarkable facts relating to cold, of which no explanation had before been attempted. The facts I mean, are those experiments in which intense degrees of cold are produced by mixture.

The mixtures most effectual for this purpose are those of ice and snow with different salts; and it always happens, that while the cold is produced, the salt and snow dissolve together into a liquid. This liquefaction of the salt and snow is absolutely necessary to the production of the cold. In order to explain this phenomenon, we need only to

recollect, that the liquefaction, which suddenly happens,
requires, as on other occasions, that a quantity of heat be
absorbed, and combined, in a latent state, with the lique-
fied matter ; that the mixture cannot assume the form of
a fluid without absorbing into it a quantity of heat, or of
the matter of heat ; and that this heat becomes latent in
the composition of the fluid that is produced, being
combined with the matter of the fluid more closely and
intimately than heat that is in its ordinary sensible and
moveable state.   This requisite heat can be had only from
the materials.   A part, therefore, of the sensible heat,
which was in the materials before they were mixed, sud-
denly assumes that form, and disappears, so as to be no
longer perceptible by the feeling of heat, or by means of
the thermometer.   The mixture, therefore, appears much
colder than the materials were immediately before. More-
over, we see in this example the converse of what happens
in Fahrenheit's experiments with over-cooled water.   In
these cold mixtures we make a quantity of ice melt sud-
denly, without adding heat to it; and this liquefaction
occasions a great degree of cold to be instantly produced,
by the conversion of a quantity of sensible heat into latent
heat.   In Fahrenheit's experiment, we occasion a quan-
tity of water to freeze suddenly, by disturbing it only, and
without taking heat from it. A quantity of heat suddenly
appears in it, or is suddenly changed from a latent to a
perceptible state.

To understand better the power of these mixtures in
producing cold, and to what extent they can produce it,
we must recollect, that salt added to water increases con-
siderably its disposition to fluidity but increases it only to a
certain degree.   In a strong brine of common salt, for ex-
ample, the disposition of the water to fluidity is so much
greater than in pure water, that the brine will bear to be
cooled down to the beginning of Fahrenheit's scale, or even
a few degrees lower, without shewing any beginning of
congelation.   But if, by exposing to the action of a suffi-
ciently powerful cooling cause, we cool it a little more, for
example, to five or six degrees below Zero, then the chemi-
cal attraction of the salt and water for one another begins

to be overcome by the cohesive attraction of each of these substances : a small part of the salt concretes into grains like sand, and falls to the bottom, and a small part of the water freezes. This is accompanied by the extrication and emergence of a proportional quantity of latent heat, or the conversion of it into sensible heat, which increases the sensible heat of the brine a little, and puts a stop to the farther separation of the salt and congelation of the water, until this small addition of sensible heat be taken away again by the cooling cause. But as soon as it is taken away, a little more of the salt concretes, and a little more of the water freezes. All which repeatedly happens, until at last, the whole of the latent having been gradually extricated, the salt will be found at the bottom, all in small grains, and the water over it all congealed.

From this we can explain the peculiar phenomena of some of these experiments. For example, it is remarkable of the mixture of ice and common salt, that it never produces a cold greater than Zero, or a few degrees below Zero, but that it continues cold to this degree for a considerable length of time, during which the salt and snow are gradually melted. Now it must necessarily happen, that as soon as they begin to melt, so much of their heat will be converted into latent heat, by the melting of a small part of these materials, that their remaining sensible heat will be greatly diminished. It will be only sufficient for heating them to the degree Zero on Fahrenheit's scale ; but, when the remaining solid ice and salt are cooled to this degree, they cannot act further on one another by chemical attraction. The cohesive attraction of each is become too strong to admit of this; they cannot act further, until a little more heat be communicated to them from the surrounding bodies. As soon as this happens, they again act, and a small part of them melts, which again brings down their sensible heat to the low level; therefore they must necessarily unite together, and melt, with a very slow progress, regulated by the gradual communication of heat from the surrounding bodies, which heat, as fast as it is communicated, is changed into latent heat, and the sensible heat of the mixture must continue at the same low de-

gree, so long as any considerable part of the ice and salt remains unmelted. Some other salts increase, to a still greater degree, the disposition of water to fluidity, and will therefore produce a much greater degree of cold in this way.

It will not be unacceptable, I presume, on this occasion, to mention the most successful and convenient processes for producing great diminutions of heat, or, as it is more commonly expressed, for producing intense colds. This is not a matter of mere curiosity, but is frequently the means of philosophical investigation ; and some very interesting facts have been discovered in this way.

I think you are now sufficiently aware, that, since fluidity is the consequence of a certain combination of calorific matter with the substance of solid bodies, it must follow, that if any substances are induced, by their chemical properties, to act on each other, so as mutually to dissolve and become fluid ; and if there be no external source, from which the necessary latent or fluid-making heat can be obtained, it must be furnished by the materials themselves ; that is, some of their sensible heat must disappear, their temperature, indicated by the thermometer, must be lowered; and this must happen in a degree so much the more remarkable, as there is more of the solid matters rendered fluid, and also, in proportion as the substances so liquefied, require a greater quantity of latent heat for this purpose. An ounce of one substance may thus absorb as much as two ounces of another substance.

You can easily see further, and I have already mentioned some examples of it, that, whatever are the substances employed, there must be a limit to the diminution of temperature. If the liquid mixture congeals at any particular temperature at 40° for example, it is impossible to produce a greater cold by mixing those substances.

These observations, therefore, suggest certain maxims or rules for the powerful operation of chemical mixtures for producing intense colds. We must choose substances, whose chemical action induces them strongly to dissolve each other in a liquid form. They should also be such as require a great quantity of latent heat ; and they must be such whose mixture will bear a very great diminution of

temperature before it freezes. I cannot say that these circumstances have been made the subject of particular inquiry, so as to enable us, *a priori*, to make the most effective selecton of materials. It has generally been by random and numerous trials.

There is another maxim which may be deduced from the knowledge we have acquired. When a certain substance requires a determined quantity of latent heat for its appearance in a liquid form, it is plain, that when other circumstances are the same it will produce, not a certain determinate temperature, but a certain determinate diminution of temperature.

Of this consequence we can make a most effective use. Suppose that a certain mixture produces a diminution of 20 degrees, and that it is made in the temperature of 50, it will acquire the temperature 30 degrees, and no lower. But we can employ this temperature to cool another quantity of the same ingredients separately to the tempearture of 30 degrees. Let these ingredients be mixed: we shall then produce the temperature 10 degrees; and if this be employed in the same manner, to cool another parcel of the same ingredients, their mixture will produce the temperature 10°. By the repetition of this process, we shall at last reach the utmost cold that is producible by those ingredients.

I have just to observe further, that as, during this process, the external air, must be continually imparting heat to our mixtures, and this with rapidity, when the cold already produced is very intense, it is necessary to cut off this supply as much as possible. The experiments should be made when the air is very cold. The vessels should be set in other vessels, made of bad conductors of heat ; and the interval should be occupied by furs, or down, or other spongy matters.

I thought it best to give you these general principles and maxims, deduced from the knowledge we have acquired, that you may perhaps fall upon more effective methods than any I can relate or describe.

The substances generally employed have been salts
dissolved in water.   None has been found more effica-
cious than sal ammoniac. If three parts of water be added
to one part of this salt, the liquefaction lowers the tempe-
rature fully 20 degrees of Fahrenheit.   We can therefore
freeze a little cold water when the air is at 48°, the tempera-
ture of ordinary fountains in this country. Two such opera-
tions would freeze a little water even in our ordinary sum-
mer heat.

A much shorter and effective process was discovered
by Mr. Walker, apothecary, in Oxford.   He mixed 11
ounces of sal ammoniac, 10 of saltpetre, and 16 of Glau-
ber's salt.   He then poured on this mixture of dry salts
32 ounces of water, stirring the mixture, to accelerate their
solution.   Thus he produced at once a diminution of 48
degrees of Fahrenheit.   The experiment being made when
the temperature of the materials was 65°, it was changed
to 17°, which is 15 degrees below frost.

He produced still greater colds, by mixing some salts
with acids.   Three parts of aquafortis mixed with 12
of Glauber's salt, sunk the thermometer 52 degrees : and
then, adding six parts of sal ammoniac, he sunk it eight
degrees further.   He froze quicksilver, by repeating this
process.   The vitriolic acid also, diluted with an equal
weight of water, produced intense cold, by dissolving crys-
tallized Glauber's salt.   It sunk the thermometer (then
very low) 46 degrees.

Dr. Guthrie writes me from Petersburgh, that Mr.
Lowitz, in December 1792, when the thermometer was at
60 degrees of Fahrenheit, froze completely four pounds of
quicksilver, by the mixture of dry snow with crystallized
caustic potash.   As the preparation of this salt is very
troublesome, and as it corrodes every thing it touches, and
is dangerous for the operator, he tried another salt, called
fixed-ammoniac, which is the residuum in the manufacture
of caustic volatile alkali, and is very cheap.   When this
is evaporated, till a drop of it on a plate fixes immediately,
it is then allowed to cool and grow solid.   It attracts wa-
ter with greediness, and must be kept from deliquidating, in
well corked bottles.   This salt, mixed with dry snow, is

very nearly as powerful as the potash, and also froze the quicksilver. It is, perhaps of all others, the most generally convenient: it is cheap, and easily preserved fit for this use: and when used, it will serve again as well as ever, by evaporating the water produced in the experiment. Pounded ice may be used instead of snow in summer ; but it must be inferior in efficacy, because there adheres to it some water already provided in latent heat. Mr. Lowitz found that both of these salts were twice as powerful in producing cold as smoking nitrous acid, which produces the depression 41 degrees. He found that these salts produced about 80 degrees. If these numbers have been accurately reported to me, this method of Mr. Lowitz's is the most efficacious that I know*.

As soon as the solution ceases, the cold begins to abate, by the influence of external warmth. The process must therefore be conducted expeditiously, and with the defences mentioned above. Ice and common salt, melting slowly, produce a cold of considerable continuance and intensity. But I do not imagine that it can be lower than about——5° of Fahrenheit, because, about that temperature, the salt, being no longer attracted by the water, begins to crystallize and give out its latent heat. I am uncertain what may be the limits of those other mixtures.

Thus I have given the facts relating to the melting of ice and freezing of water, connecting them together, so as to lead to the same conclusion ; and I have communicated the notions and reasonings which I founded on those facts. I must now add, that the same notions and reasonings may be applied to the liquefaction of many and, probably, of all other bodies by heat : at least, a great number exhibit appearances when they are liquefied, and when they return from the fluid to the solid state, very similar to those exhibited by ice or by water, when undergoing these changes.

The late ingenious Dr. Irvin, who was my pupil at Glasgow, and afterwards my successor in the chemical chair, making some experiments, at my desire, on the

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* I find the same numbers in the account given by Crell in his Chem. Annals, 1793. 1. 352.   EDITOR.

melting and congealing of spermaceti, and bees-wax, and some other substances, found that the spermaceti in melting, absorbed a quantity of heat, which, though it did not make it sensibly warmer, but only fluid, would have been sufficient for making melted spermaceti warmer by 141 or 148 degrees.

Bees wax absorbed a quantity equal to what is sufficient for making melted bees wax warmer by 175 degrees. And from experiments with tin, he found reason to conclude, that this metal, while melting, absorbs a quantity of heat equal to what might add 500 degrees of heat to it in its solid state.

It appears also from Dr. Guthrie's experiments on the congelation of quicksilver*, that it absorbs a great quantity of heat, while it is changed by heat from a solid to a fluid. A cylinder of congealed quicksilver, adhering to the bulb of a thermometer, being carried into a very warm room, very soon began to melt; but it required a considerable time to melt it completely, the thermometer involved in it pointing all the while to the——40th degree of Fahrenheit. As the quicksilver all this time was so very much colder than the air around, it must have received heat in a great quantity from that air.

Appearances also occur in the refining of copper and of silver, and the melting and moulding of phosphorus, which not only shew that those substances, when melted, contain a quantity of latent heat, but that, in certain circumstances, they can, like water, retain this latent heat until they are cooled considerably below their congealing point. Another opportunity will be taken of mentioning some of these facts, which are very curious, but cannot be fully explained without a previous knowledge of the peculiar properties of those substances. I am persuaded also, that the great and sudden contractions of the quicksilver in Professor Braun's thermometers, depended on the same circumstances. The quicksilver was cooled beyond the degree sufficient for its congelation, yet still retained its latent heat and fluidity, but being then disturbed, was made

* Phil. Trans. 1786.

to congeal, at least in part, and the congelation of it is at-
tended with extraordinary contraction*.

There is reason to think, that not only the fluidity of
bodies, but that even the softness of such as are softened
and rendered plastic by heat, depends on a quantity of heat
combined with them in the form of latent heat. In the
experiments made by Dr. Irvin on the congelation of
bees wax, it was quite plain, that after the wax had lost
its fluidy, and while, from its soft state, it slowly became
harder, till it acquired its greatest hardness, a quantity of
latent heat was gradually extricated, and, assuming the
form of sensible heat, kept up the heat of the cooling wax
much longer, and occasioned its abating much more slow-
ly than it would otherwise have done. The doctor was
surprised at it, and was at a loss to account for it, until I
pointed out to him the cause that appeared the most pro-
bable, which was, that the soft state of wax is produced by
a certain quantity of latent heat in it, through a much less
quantity than that necessary to give it fluidity.

On account of this, and some other facts, I began to
suspect, that the malleability and ductility of metals depend
on the same cause ; and, after mature reflection, I am per-
suaded that this is the case. While metals are hammered,
and extended under the hammer, they become warm, and
in some cases very hot ; but at the same time they become
very rigid, and are no longer malleable. If we attempt

† Dr. black, in several courses of his lectures, extended this doctrine
much farther, namely, to the cases of solution of salts in water, and several
others, when solid bodies are rendered fluid by the action of proper fluids
upon them ; as also to some cases where fluid bodies become solid by che-
mical action, as in the churning of butter, the formation of cinnabar, &c.
Some of these are exceedingly curious, and greatly puzzled the philosophers.
Thus, if a saturated solution of Glauber's salt, in hot water, be allowed to
cool in a place where it suffers no agitation or tremor, it will frequently
grow cold without crystallizing. If it be now shaken, it crystallizes sudden-
ly, and the vessel becomes very warm, and even hot. In the combination
of mercury with sulphur, by the assistance of heat to melt the sulphur,
when the true chemical union takes place in a considerable quantity at once,
it becomes stiff on a sudden, and heat emerges sufficient for kindling the su-
perfluous sulphur·

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to beat them out farther, they are sure to crack at their edges, not having the same toughness and softness as before. The only way now to restore that toughness is by annealing them, that is, by making them hot in the fire, and allowing them to cool. This being done, they are found malleable again, but liable to lose this malleability by a second hammering, and they need the introduction of heat into them again, if we desire to hammer them more.

The following experiment gave me a proof of that. In order to anneal them, the single circumstance of their being heated is not sufficient, but it is necessary that the heat be communicated to them from other matter. I desired a smith to make iron red hot by hammering it. He very soon did it by hammering the extremity of a rod of iron properly prepared. It assumed a moderate, or dull red heat under the action of the hammer. I desired him to let it cool, and then hammer it again, to make it red hot a second time. He told me this could not be done without first annealing it, or softening it *in the fire*; and that if he should attempt to hammer it again without this preparation, it would not bear the strokes of the hammer without splitting and breaking into pieces, of which he satisfied me a little while after. But when the iron was in this brittle state, it needed only to be heated in the fire until it had a dull red heat, similar in appearance to that which had been produced in it by the strokes of the hammer; being then allowed to cool, it was found malleable. It is plain, that if nothing were requisite for annealing the iron in this experiment, but making it red hot, and allowing it to cool slowly, this is already done; for the iron is left red hot by the hammer.

I therefore consider the metals as substances, which have the power to retain strongly a certain quantity of latent heat, which gives them their toughness and malleability, but I imagine that this heat is driven out of them by the violent agitation, compression, and friction of their parts, in hammering them strongly into another shape. Those called the more perfect metals retain this heat with the greatest force, and retain it in some cases, though extended

by skilful hammering to an amazing degree. Tough iron, which is a purer metal than steel, contains more of it than steel does, and shews a little more power to retain it; from iron it cannot be expelled but by the strokes of the hammer, or violent compression ; from steel it can be seperated not only by hammering, but also by sudden and violent refrigeration of the steel from a red hot state. This happens in the operation called the hardening of steel; The steel is made red hot in the fire, and then suddenly plunged into cold water ; thus it is made excessively hard, but at the same time perfectly inflexible and brittle. We must therefore conclude, that this sudden and violent refrigeration prevents its retaining a due portion of latent heat, which it would have retained, had it been allowed to cool slowly and quietly. Iron, when treated in the same manner, loses but very little of its latent heat.

And now I have given, what appears to me a just account of the nature of fluidity and softness, considered as general effects of heat, and of the manner in which these qualities are produced by heat ; together with a number of facts belonging to this subject, from which it may be perceived, that when properly understood, they are connected together, and explain one another, as well as some other facts of which no explication had been offered before.

But since I have been accustomed to communicate this view of the subject, attempts have been made to explain some of the facts a little differently. It has been imagined, that in order to account for the phenomena, I suppose a cause which is not at all necessary, namely, that it is a special combination of the substance of bodies with the material cause of heat, which, while it continues in this state of combination, I call *latent heat*. This supposition has been said to be unnecessary, because the appearances can be explained by another principle, which, it is said, is an uncontrovertible fact, which must necessarily produce the very appearances that we observe.

The alledged fact or principle is this : A body, while in its fluid state, has a greater capacity for heat, or absorbs more heat in order to rise one degree in its temperature,

than when it is in the solid form. Thus, for example, ice, it is said, has less capacity for heat than water has. When ice, therefore is changed into water, a quantity of heat must go into this water, without making it sensibly warmer ; and when water is changed into ice, a quantity of heat must come out of the freezing water as fast as the ice is formed, and this without leaving it sensibly colder. The absorption of heat, therefore, into the melting ice, is not the cause of its liquefaction, but rather the consequence of it ; while on the other hand, the extrication of what I call latent heat from the freezing water, is not the cause of its becoming solid, but the consequence of it.

This way of conceiving the subject occurred first to Dr. Irvin. I never had an opportunity to learn the terms in which he stated this opinion. I have here stated it in the manner in which I understood it from report, and from what Dr, Cawford afterwards published*.

But to this statement I answer, that the alleged fact of disparity between the capacity of ice and water for heat, may indeed be supposed to account for the thermometrical phenomena just now recited ; but the principal fact or phenomenon is not accounted for by it, I mean the change of the solid into the fluid. Solidity, we all know, depends on cohesive attraction ; but on what cause does fluidity depend ? Will it be said to depend on the absence or cessation of cohesive attraction ? I cannot imagine how cohesive attraction can entirely cease, or be suspended,

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* It has already been mentioned, when treating of the unequal distribution of heat, how Dr. Irvin was led to attend very minutely to the different capacities of bodies for heat ; and his notion of the heat, which appeared when we mix vitriolic acid, in its most concentrated state, with water, has also been noticed. This heat is frequently such as to be insupportable by the hand, and splits the glasses in which the mixtures are made. It is not wonderful, that Dr. Irvin should think this sufficient to account for the slow extrication of 140 degrees of heat. It must be added, that the few experiments which this temperate climate allowed him to make, on the comparative heats of ice and water, and the low temperature which he thought was the commencement of absolute heat, authorised him to say, that the sum total of the excesses of the specific heat of water, above those of ice, was more than sufficient to account for all the heat that emerges in freezing, or is absorbed in melting.

or, if this should happen, how it should easily be again restored : Nay, we have evident proof, from the round form of the drops of liquids, that it is not in them entirely suspended, but only weakened to such a great degree, that the solid hard body is become a liquid. Now this is such a violent change, that I cannot help thinking it must depend on some powerful cause. It may be said, perhaps, that no other cause is necessary but the increase of sensible heat above a certain degree, together with some singularity in the nature of cohesive attraction; such as that this attraction, though very strong at certain small and imperceptible distances, becomes suddenly very weak. when we increase, beyond a certain limit, the distance of the particles of matter from one another, and this increase of distance is produced by sensible heat.

If this were true, the increase and diminution of distance by the action of sensible heat, ought always invariably to produce, each its appropriated effect, on the power of the cohesive attraction ; as water, when its degree of heat is above 32°, is always liquid, so when its sensible heat is below 32°, it should always be solid. But this we know is not always the fact. Water, in some circumstances, can be cooled to 7 or 8 degrees below 32° without being congealed ; and many other substances exhibit the same phenomenon, by retaining their fluidity in some circumstances, after their sensible heat is diminished considerably below their ordinary congealing point. And I now ask, what is the cause which hinders the cohesive attraction from producing its effect, and changing the liquid into a solid ? When I find by experience, that, upon disturbing such over-cooled liquids, a quantity of heat is extricated from them, which did not appear immediately before, and that, while this heat is extricated, a proportional part of the liquid congeals ; I cannot help considering this latent heat which was in it, as having been the cause of its protracted fluidity.

We have also, in the experiment on iron above described, a striking example of the difference between sensible and latent heat, and of the want of power in the one to produce the effects of the other. When the latent heat of the

soft iron was extricated by hammering, and changed into sensible heat, it had no longer the effect to give toughness and malleability to the iron, but the greatest part of it was communicated to the surrounding bodies, as sensible heat always is; thus leaving the iron deprived of the circumstance necessary to its toughness. Moreover, by no other means can this quality be again restored to the iron, but by giving it an opportunity to recover in the fire, and retain, in a state which does not affect the thermometer, this quantity which had been expelled from it. In which process of annealing the iron, it is not necessary to heat it to a greater degree of redness than that which was produced in it by the eruption of its latent heat.

I am, therefore, still of the same opinion that I mentioned to you a little ago upon this subject. I consider fluidity as depending, immediately and inseparably, on a certain quantity of the matter of heat which is combined with the fluid body, in a particular manner, so as not to be communicable to a thermometer, or to other bodies, but capable of being extricated again by other methods, and of re-assuming the form of moveable or communicable heat.

We now see the reason for the sixth caution given for conducting the experiments for determining the specific capacities of bodies for heat. If they act on each other, so as to produce the solution, or even the softening, of one of the bodies, heat will be absorbed; or, if the mere heat of the mixture melts or softens either of them. For the same reason, we cannot examine the temperature of intense heats in Newton's method, when the body examined freezes during its exposure to the stream of cold air; nor can we discover it by pouring the melted body into water, if the temperature produced be so low that the body freezes. *(See Note 4. at the end of the Volume.)*

SECT. III.

OF VAPOUR AND VAPORISATION.

HAVING considered, in sufficient detail, the appear-
ances which are exhibited when solid bodies are melted
by heat, and having shewn the connection which the dif-
ferent circumstances accompanying this change have with
the general operation of heat, we now proceed to treat of
another, and more remarkable change, produced by this
great agent in nature; I mean the conversion of bodies
into an elastic vapour, that is, into a fluid extremely rare,
light, and expansive, like air, capable, like it, of being
easily reduced into less space, by external pressure, and
resisting, like it, the force which thus compresses it.
That you may conceive this more distinctly, I shall direct
your attention chiefly to this operation of heat as it affects
fluid bodies, and, taking water as the most familiar in-
stance, I shall describe the manner in which it suffers this
change.   Suppose a single tea-spoonful of water to be
put into a globe of glass, or metal, capable of holding
some gallons, and suppose this vessel exhausted of air.
If we apply heat to the globe, we shall perceive the water
gradually to disappear, so that presently the globe shall
seem empty.   But the fact is, that it is completely filled
with the water, now existing in the form of a perfectly
transparent vapour; for, if the heat be still farther in-
creased, the expansive force of what now fills the globe
will also increase to such a degree, as even to burst it.

Upon this change of water from its ordinary state into
that of a rare expansive fluid, depend the effects of what
are called candle bombs.   These are hollow spherules of
glass, formed in the lamp, on the end of slender tubes.
A little bit of the tube is left adhering to the ball.   By
holding the ball in the flame of a candle, part of the air
is driven out by expansion.   The end of the tube is then
dipped into water, and, as the air in the ball collapses

again by cold, the water is forced into it by the pressure of the atmosphere, till it is perhaps half full. The tube is then sealed hermetically. If this little ball be stuck into the wick of a burning candle, the water is converted into elastic vapour, which bursts the glass with a loud crack. As the vapour expands in all directions, the candle is blown out, and the wick is commonly beaten down, as if by a blow with a hammer.

In like manner, the æolipile affords a very plain proof of the great rarity and elasticity of the vapour into which water is converted by heat. The æolipile is a hollow ball of copper, having no opening but a narrow pipe, which is bent at the top, nearly at right angles. It is partly filled with water, in the same manner as the candle-bomb. If it be now set on burning coals, with its pipe pointing into the midst of the fuel, we shall soon perceive a violent blast proceed from it, which, when properly managed, will blow up the fire like a pair of bellows. This will continue till the water is all expended. We shall afterwards learn that it blows up the fire, chiefly, because the vapour of the water drags along with it the surrounding air, and forces it through the interstices of the fuel.

It is partly owing to this conversion of water into elastic vapours that it is so dangerous to allow it to come near hot oils. The boiling of lintseed oil, for example, is always done without doors, to prevent accidents; and we are obliged to choose a dry day, because a drop of rain falling among the oil would sink down through it, and would be suddenly converted, by the excessive heat of the oil, into a great bulk of expansive vapour, which would dash the oil out of the vessel into the fire. I said that this vaporisation of the water is *partly* the cause of such accidents, because it is known, of late years, that the water is by this means converted into another expansive fluid, much more bulky and elastic than the mere hot vapour of water. Of this you will learn more afterwards. It is the water which all oils contain, in a small quantity, that is the cause of their first frothy boiling, after which they remain quiet, till raised to a vastly higher temperature. In like manner, it

OF VAPORISATION.

is dangerous to admit water, or even dampness, into the
moulds in which metals are cast. In the casting of iron,
they form the moulds of sand, which being very porous,
allow the humidity to escape without any violence. When
copper is melted in very large masses, it contains a prodi-
gious quantity, both of sensible and latent heat, which gives
it a great power of converting water into expansive va-
pour. It has sometimes happened that a person, by care-
lessly spitting in a copper foundry, has occasioned an ex-
plosion that destroyed the whole building.

The bulk into which water expands, by the ordinary
boiling heat, producing a steam equally elastic with com-
mon air, is very great. It has been, however, much over-
rated by some very respectable authors. Dr. Desaguiliers,
on the authority of Mr. Nieuwentyt, and some of his
own experiments, says that it expands into 14000 times
its natural bulk. But, by my own experiments, and those
of my friend Mr. Watt, whose interest in this matter made
him uncommonly accurate, its greatest expansion in
these circumstances, does not exceed 1800 times its former
bulk.

This change is produced, like fluidity, in consequence of
the heat of the body being increased beyond a certain
temperature. This is found to be different for almost
every different kind of matter.* Like fluidity, also, this
temperature must be continued, in order that the vaporous
form may remain. If the heat be again diminished the va-
porous form is destroyed, the vapour loses its elasticity, and
again collapses into that form from which it was produced.
You must all have observed this in the case of water, the
steam of which is condensed into water again, whenever it
strikes any mass of cold matter, such as a lump of iron.

In general, therefore, the conversion of any particular
kind of matter into vapour, depends on its being heated
above a certain degree, and this vapour collapses again, as

* The earliest notice that I can meet with of this observation, is that by
Dr. Hooke, who proposed the temperature of boiling water, as a fix d point
in the scale of the thermometer. This was in 1684. *See Birch's Hist. of
the Royal Society*, vol. iv.   EDITOR.

soon as its heat is diminished below that degree. This degree, or vaporific point of heat, is found (with respect to most bodies) to be higher than that which is necessary for giving them fluidity. Almost every particular kind of matter requires a different temperature for this purpose.

Hence the chemical terms of VOLATILE and FIXED. Those bodies which are most easily converted into vapour are generally called volatile, and this disposition to vaporisation is called VOLATILITY. Those bodies, on the other hand, are called FIXED, which are with greater difficulty converted into vapour, or require a higher temperature for this conversion. Sometimes, indeed, the term fixed, or fixedness, has been understood to mean a total want of volatility, or capacity of being converted into vapour by any degree of heat. But this is not the true meaning, and it is doubtful whether any body in nature is unsusceptible of vaporisation by heat. These terms, therefore, are only terms of comparison, and a fixed salt is not a salt incapable of vaporisation by heat, but one that requires a high temperature for its conversion.

I have represented the degree of heat at which any particular kind of matter begins to be converted into elastic vapour, as steady or determinate, in the same manner as the degree which gives it fluidity. I must now restrict this notion, so far as to observe that it is steady, only when bodies are heated under the same circumstances. But there are some circumstances, which, when they are varied, occasion very great differences in the vaporific temperature of every kind of matter. This point, therefore, is not so unchangeable as the melting point. So far as experience goes, the melting point of each kind of matter is always the same; nothing but a certain invariable intensity of heat can give fluidity to a particular solid substance, nor can any variation of circumstances hinder that solid from becoming fluid, when raised to the melting point, and a proper quantity of heat is communicated to it. But the change of a body into vapour is affected by pressure, as well as by heat. In proportion as the external pressure

upon its surface is greater, it endures a greater heat, without assuming the vaporous form, than it would otherwise do; and thus we may say, that vaporisation is retarded or opposed by external pressure.

This was first observed, I think, by Boyle, when making experiments with his air pump. Some time after this, Fahrenheit, when graduating his thermometers, observed that the degree of heat which produces elastic vapour from water, and is called its BOILING POINT, was liable to a little variation at different times, even when examined with the same thermometer. Attending to all the circumstances that accompanied these variations, he found that they always corresponded with the changes of the weight, or pressure of the atmosphere. In future, therefore, he always attended to the height of the barometer, when he marked the boiling point upon his thermometers. The thing was soon very well known, and the variations of the boiling point, corresponding to the heights of the mercury in the barometer, were noted with precision by those who were interested in this subject. As the changes in the barometer are not very great, so the variations in the temperature of boiling water are but inconsiderable, never exceeding two or three degrees in these countries. (See Dr. Martin's *Essays on Heat*——Sir Geo. Shuckburgh *on the Variations of the Boiling Point*——*Phil. Trans.* vol. 69.)

The variations of the boiling point of fluids, by still greater changes of external pressure, have been more particularly considered by different writers, and machines have been contrived where their effects have been much more conspicuous. The most remarkable of these is the vessel called a DIGESTER, the invention of Mr. Papin, a very ingenious French physician, residing in London. Papin's digester is a copper vessel, generally cylindrical, having a lid nicely fitted to it, and kept fast by screws. It is usual to interpose a slip of soft paper between the digester and its lid, to prevent the elastic vapour from forcing its way out. If this vessel be half filled with water, and the lid, screwed tight down, and if it be then set upon burning

coals, a portion of the water is soon converted into STEAM
(this is the name given to the elastic vapour of boiling
water.) This conversion begins in the boiling tempera-
ture of the water; but the elastic vapour being confined, it
presses on the surface of the water, and thus prevents the
conversion of any more of it into steam, But the conti-
nued application of heat raises the temperature of the wa-
ter above its ordinary boiling point, and occasions the con-
version of a small portion more of it into vapour, whose
elasticity is at least equal to that of the steam already ex-
isting. And thus the production of steam is again stop-
ped till the heat be raised still higher, and the elasticity of
the steam be still farther increased. We are not very
well informed concerning this internal procedure, nor very
certain whether any second, or subsequent, portion of wa-
ter is thus converted into steam, by the increase of heat.
This must depend upon the rate at which the increase of
heat augments the elasticity of the steam already produ-
ced.

We know, however, that by the confinement of the
steam, and the consequent increase of the pressure upon
the surface of the water, it may be made to endure very
considerable degrees of heat. The ingenious inventor
employed it chiefly for increasing the dissolving power and
general activity of water, and watery fluids, in various oper-
ations of cookery, pharmacy, and other chemical arts.
He thus enabled water completely to dissolve horn, tor-
toise-shell, cartilage, and even bone, into a jelly. He
made many extracts, which could not be obtained in any
other manner. These effects induced him to call his ap-
paratus a digester. Mr. Muschenbroeck found, that a
heat might be produced in the water contained in the di-
gester, sufficient for melting bits of tin and lead, that were
hanging by wires in the midst of it. These are very haz-
ardous experiments, and terrible accidents have sometimes
happened, by the bursting of the digester.

This variation in the temperature, necessary for the con-
version of bodies into elastic vapour, is not peculiar to wa-
ter, but obtains universally. Seeing, however, that the

pressure of the atmosphere does not vary very considerably in any one place, where observations are made, the vaporific temperature of water and other bodies, when not subjected to any artificial pressure, is pretty steady. These are the temperatures which are generally considered and mentioned as their natural vaporific points. No doubt, in one sense, they are so, being the temperatures at which such bodies are converted into vapour, in the ordinary course of observation. But they are by no means the heats at which such bodies would begin to assume the vaporous form, if left to their own natural disposition alone, and exposed to no other action but that of heat. But, under the pressure of the atmosphere, every body is forced to receive much more heat than it would otherwise endure, without assuming the form of vapour. Some experiments, made by Mr. Robison, upon water, spirit of wine, mixtures of spirits and water, and sulphuric acid, shew that the difference between the temperatures at which they boil, *in vacuo*, and in the open air, is nearly the same in all, namely, 120 degrees of Fahrenheit's scale*.

As the pressure of the atmosphere does not commonly affect our senses, it does not engage our attention, and we bear it without inconvenience, or rather our constitution is such, that we cannot exist without it. But we must not overlook this when treating the subject of vaporization. We must remember that we live under an ocean of air, which covers the whole globe, to the height of about 50 miles, and which has a very sensible weight. Thus, we find that a large bottle is heavier, when filled with air, than after it has been extracted from it, a cubic foot of air weighs about 1 ^{1} ounce troy. Therefore, when we boil a fluid at the top of a high mountain, we do it under a smaller pressure than upon the sea-shore. We must attend

* These experiments were made by me in 1764, by the help of a very indifferent air-pump. I have repeated them since, in air still more rarefied, and still have reason to think that the difference of temperature is very nearly the same in all of them, but 22 or perhaps 25 degrees more than what I then found.

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to this in trying the boiling heats of different fluids, at different elevations above the sea.

Let us now attend to some other differences of circumstances and conditions, under which vapour may be produced. When a small quantity of any fluid, a drop of water, for instance, is heated very quickly, as when we throw it upon a piece of red hot iron, it is soon heated up to its vaporific point, because every part of this quantity of matter receives the heat nearly in an equal manner, and, almost at the same instant, arrives at that temperature which it cannot pass without being converted into vapour. Hence it happens that the drops run about upon the iron plate like quicksilver, for the iron communicates heat so very fast to the water, that the adjoining parts are immediately converted into vapour, which prevents the water from coming into contact with the iron. Hence also it happens, than when a large lump of red hot iron is thrown into water contained in a pewter or wooden vessel, it will melt, or burn a hole in the bottom, because the constant atmosphere of vapour around it keeps off the water all the while, and the iron is but slowly robbed of its heat.

But when we heat a large quantity of a fluid in a vessel, in the ordinary manner, by setting it on the fire, we have an opportunity of observing some other phenomena which are very instructive. The fluid is gradually heated, and at least attains that temperature which it cannot pass without putting on the form of vapour. In these circumstances, we always observe that it is thrown into a violent agitation, which we call boiling. This agitation continues as long as we throw in more heat, or any of the fluid remains, and its violence is proportional to the celerity with which the the heat is supplied.

Another peculiarity attends this boiling of fluids, which, when first observed, was thought very surprising. However long and violently we boil a fluid, we cannot make it in the least hotter than when it began to boil. The thermometer always points at the same degree, namely, the vaporific point of that fluid. Hence the vaporific point of fluids is often called their boiling point.

When these facts and appearances were first observed, they seemed surprising, and different opinions were formed with respect to the causes on which they depend. Some thought that this agitation was occasioned by that part of the heat, which was more than the water was capable of receiving, and which forced its way through, so as to occasion the agitation of boiling; others again imagined, that the agitation proceeded from air, which water is known to contain, and which is now expelled by the heat.

Neither of these accounts however, is just or satisfactory; the first is repugnant to all our experience in regard to heat; We have never observed it in the form of an expansive fluid like air. It pervades all bodies, and cannot be confined by any vessel, or any sort of matter, whereas the elastic matter of boiling water can be confined by external pressure, as is evident in the experiments made with Papin's digester.

Yet, notwithstanding the incompatibility of this explanation with our most familiar experience, it had a very general currency. No wonder that so ill-founded an opinion was accompanied by consequences and deductions equally fanciful. Even philosophers gave credit to the vulgar notion, that the vessel in which water boiled with violence has very little heat in it, because, say they, the fire runs up through it as through a sieve, without heating it; therefore, when the finger is applied to the bottom of a boiling tea-kettle, it is felt no more than moderately warm. But this is a silly mistake. The trial is commonly made with a sort of fearful hurry, the fire all the while affecting the back of the hand, and obliging us to withdraw it before we can form any distinct opinion of the matter. But the great cause of this vulgar opinion is, that the bottom of a tea-kettle is usually covered with a spongy crust of soot, which is almost the worst conductor possible of heat. If it be scraped clean, and especially if the wet finger be applied to it in that state, it will be scalded in an instant.

The second explanation is equally unsatisfactory, nor is it intelligible; for, if we continue to apply heat to the water, it will continue to boil to the last drop, and we should therefore conclude that it is all air. But to be satisfied that this opinion

is not true, we need only apply cold, or a cooling cause, to this supposed air, and we shall immediately find it condensible into the same quantity of water from which it was produced.

A more just explanation will occur to any person, who will take the trouble to consider this subject with patience and attention. In the ordinary manner of heating water, the heating cause is applied to the lower parts of the fluid. If the pressure on the surface be not increased, the water soon acquires the greatest heat which it can bear, without assuming the form of vapour. Subsequent additions of heat, therefore, in the same instant in which they enter the water, must convert into vapour that part which they thus affect. As these additions of heat all enter at the bottom of the fluid, there is a constant production of elastic vapour there, which, on account of its weighing almost nothing, must rise through the surrounding water, and appear to be thrown up to the surface with violence, and from thence it is diffused through the air. The water is thus gradually wasted, as the boiling continues, but its temperature is never increased, at least in that part which remains after long continued and violent boiling. The parts, indeed, in contact with the bottom of the vessel may be supposed to have received a little more heat, but this is instantly communicated to the surrounding water through which the elastic vapours rises.

This has the appearance of being a simple, plain, and complete account of the production of vapour, and of the boiling of fluids; and it is the only account that was given of this subject before I began to deliver these lectures: but I am persuaded that it is by no means a full account of the matter. According to this account, and the notion that was conceived of the formation of vapour, it was taken for granted that, after a body is heated up to its vaporific point, nothing further is necessary but the addition of a little more heat to change it into vapour. It was also supposed, on the other hand, that when the vapour of water is so far cooled as to be ready for condensation, this condensation, or return into the state of water, will happen at once, or in consequence of its losing only a very small quantity of heat.

But I can easily shew, in the same manner as in the case of fluidity, that a very great quantity of heat is necessary to the production of vapour, although the body be already heated to that temperature which it cannot pass, by the smallest possible degree, without being so converted. The undeniable conse-quence of this should be, an explosion of the whole water, with a violence equal to that of gunpowder. But I can shew, that this great quantity of heat enters into the vapour gradually while it is forming, without making it perceptibly hotter to the thermometer. The vapour, if examined with a thermometer, is found to be exactly of the same temperature as the boiling water from which it arose. The water must be raised to a certain temperature, because, at that temperature only, is it disposed to absorb heat; and it is not instantly exploded, be-cause, in that instant, there cannot be had a sufficient supply of heat through the whole mass. On the other hand, I can shew that when the vapour of water is condensed into a liquid, the very same great quantity of heat comes out of it into the colder matter by which it is condensed ; and the matter of the vapour, or the water into which it is changed, does not become sensibly colder by the loss of this great quantity of heat. It does not become colder in proportion to the quantity of heat obtainable from it during its condensation.

All this will become evident, when we consider with atten-tion the gradual formation of vapour, in consequence of the continued application of a heating cause, and the like gradual condensation of this vapour, when we continue to apply to it a body that is colder. Thus, if we apply a heating cause, to a quantity of water,....suppose by setting a tea-kettle with water upon the fire, the heat flows into it very fast, from the begin-ning of the experiment until the water is heated to its boiling point. Perhaps, during the last five minutes, we find that the water has increased its temperature 20 degrees. It is a mat-ter of general observation, that the passage of heat from one body to another is nearly in proportion to their difference of temperature, other circumstances remaining the same. In the present case, therefore, since the water does not sensibly change its temperature during the boiling, we may conclude

that the heat continues to flow into it nearly at the same rate, and that it receives four degrees of heat in every minute. This supposition leads to no sensible error; for I have repeatedly found that, when water rose 20 degrees during the last five minutes, it took 40 to gain 162 degrees of heat; and when I observed it to rise 38 degrees in the same time, it had risen 162 degrees in 20 minutes*. Now, if the common opinion were just, it is plain, that, in a few minutes more, the whole water would assume the form of vapour, and would produce a most violent explosion, even sufficient to blow up the house. Nothing like this happens; for, after several minutes have elapsed, we shall find that only a very small quantity of water has become vapour, and that a great while indeed must elapse, and a continual application of the fire must be made, before the whole of it can be made to assume that form. In the ordinary course of such experiments in this climate, it requires about six times the number of minutes to boil off a small quantity of water, that it takes to bring it to boil. I can scarcely remember the time in which I had not some confused idea of this disagreement of the fact with the common opinion: and I presume that it has come across the mind of almost every person who has attended to the boiling of a pot or pan. But the importance of the surmise never struck me with due force, till after I had made my experiments on the melting of ice. The regular procedure in that case, and its similarity to what appears here, encouraged me to expect a similar regularity in the boiling of water, if my conjecture was well founded. But almost every time I thought of it, it appeared to me so difficult, if at all possible, to procure a supply of heat that should be tolerably uniform, or to ascertain its irregularities, that I was discouraged from making an experiment which would in all probability be so anomalous. But I was one day told by a practical distiller, that, when his furnace was in good order, he could tell to a pint, the quantity of liquor that he would get in an hour. I immediately set about boiling off

* In both of these cases, the heat had entered somewhat faster in the beginning of the experiment, while the temperature of the water was lower.

small quantities of water, and I found that it was accomplished in times very nearly proportional to the quantities, even although the fire was sensibly irregular.

I, therefore, set seriously about making experiments, conformable to the suspicion that I entertained concerning the boiling of fluids. My conjecture, when put into form, was to this purpose. I imagined that, during the boiling, heat is absorbed by the water, and enters into the composition of the vapour produced from it, in the same manner as it is absorbed by ice in melting, and enters into the composition of the produced water. And, as the ostensible effect of the heat, in this last case, consists, not in warming the surrounding bodies, but in rendering the ice fluid; so in the case of boiling, the heat absorbed does not warm surrounding bodies, but converts the water into vapour. In both cases, considered as the cause of warmth, we do not perceive its presence: it is concealed, or latent, and I gave it the name of LATENT HEAT.

I shall now describe a few of the experiments, which, I apprehend, will fully establish the justice of my conjecture.

Experiment 1st. I procured some cylindrical tin-plate vessels, about 4 or 5 inches diameter, and flat-bottomed. Putting a small quantity of water into them, of the temperature 50 degrees, I set them upon a red hot kitchen table, that is, a cast-iron plate, having a furnace of burning fuel below it, taking care that the fire should be pretty regular. After four minutes, the water began sensibly to boil, and in twenty minutes more, it was all boiled off. This experiment was made 4th October 1762.

Experiment 2d Two flat-bottomed vessels, like the former, were set on the iron plate, with eight ounces of water in each, of the temperature 50°. They both began to boil at the end of three and a half minutes, and in eighteen minutes more, all the water was boiled off.

Experiment 3d. The same vessels were again supplied with twelve ounces of water in each, also of the temperature 50°. Both began to boil, at the end of six and a quarter minutes, and the water was all boiled off, from the one in twenty-eight minutes, and from the other in something more than twenty-nine.

I reasoned from these experiments in the following manner: the vessels in the first experiment received 162 degrees of heat in four minutes, or 40½ degrees each minute. If we, therefore, suppose that the heat enters equally fast during the whole ebullition, we must suppose that 810 degrees of heat have been absorbed by the water, and are contained in its vapour. Since this vapour is no hotter than boiling water, the heat is contained in it in a latent state, if we consider it only as the cause of warmth. Its presence is sufficiently indicated, however, by the vaporous or expansive form which the water has now acquired.

In experiment second, the heat absorbed, and rendered latent, seems to be about 830.

In the third experiment, the heat absorbed seems to be somewhat less, viz. about 750. The time of rising to the boiling heat, in experiment third, has nearly the same proportion to that in experiment first, that the quantities of water have. The deficiency, therefore, in the heat absorbed, has been probably only apparent, and arising from irregularity in the fire. Upon the whole, the conformity of their results with my conjecture was sufficient to confirm me in my opinion of its justice. In the course of further experiments made both by myself and by some friends, and in which the utmost care was taken to procure a perfect uniformity in the heat applied, the absorption was found extremely regular, and amounted at an average to about 810 degrees.

There are other cases where this absorption appears in a much more singular manner. I put into a very strong phial, about as much water as half filled it, and I corked it close. The phial was placed in a sand-pot, which was gradually heated, until the sand and phial were several degrees above the common vaporific point of water. I was curious to know what would be the effect of suddenly removing the pressure of the air, which is well known to prevent water from boiling. The water boiled a very short while, but the ebullition gradually decreased, till it was almost insensible. Here the formation of more vapour was opposed by a very strong pressure proceeding from the quantity of vapour already accumulated, and

confined in the upper part of the phial, and from the increased
elasticity of this vapour, by the increase of its heat. When
matters were in this state, I drew out the cork. Now, accord-
ing to the common opinion of the formation of vapour by
heat, it was to be expected that the whole of the water would
suddenly assume the vaporous form, because it was all heated
above the vaporific point. But I was beginning by this time to
expect a different event, because I could not see whence the
heat was to be supplied, which the water must contain when
in the form of vapour. Accordingly, it happened as I ex-
pected ; a portion only of the water was converted into vapour,
which rushed out of the phial with a considerable explosion,
carrying along with it some drops of water. But, what was
most interesting to me in this experiment was, that the heat of
what remained was reduced in an instant to the ordinary boil-
ing point. Here, therefore, it was evident that all that excess
of heat which the water had contained above the boiling
point, was spent in converting only a portion of it into vapour.
This is plainly inconsistent with the common opinion, that
nothing more is necessary for water's existing in a vaporous
form under the pressure of the atmosphere, than its being
raised to a certain temperature. The experiment makes it
more probable, that if the influx of heat could at that instant
have been prevented, it would have remained in the form of
water, although raised, in a very sensible degree, above the
boiling temperature.

I was anxious to learn whether the heat which disappeared
in this experiment was in an acurate proportion to the quan-
tity of vapour produced, or the quantity of water that had dis-
appeared. But the drops of water that were hurried along by
the explosion, without being converted into vapour, made it
impossible for me to ascertain this with any tolerable accu-
racy, although I repeated the experiment several times.

This experiment was afterwards made by my friend Mr.
Watt, in a very satisfactory manner. His studies for the im-
provement of his steam-engine, gave him a great interest in
every thing relating to the production of steam. He put
three inches of water into a small copper digester, and, screw-

ing on the lid, he left the safety-valve open. He then set it on a clear fire of coaks, and after it began to boil and produce steam, he allowed it to remain on the fire half an hour, with the valve open. Then taking it off the fire, he found that an inch of water had boiled away. In the next place, he restored that inch of water, screwed on the lid, and set it on the fire; and as soon as it began to boil, he shut the safety-valve, and allowed it to remain on the fire half an hour as before. The temperature of the whole was many degrees above the boiling point. He took it off the fire, and set it upon ashes, and opened the valve a very small matter. The steam rushed out with great violence, making a shrieking noise for about two minutes. When this had ceased, he shut the valve, and allowed all to cool. When he opened it, he found that an inch of water was consumed.

It is reasonable to conclude from this experiment, that nearly as much heat was expended during the blowing of the steam-pipe, as had been formerly expended in boiling off the inch of water. For, before opening the valve, the temperature was many degrees above the boiling point, and all this disappeared with the vapour. The same inference may be drawn from the time that the digester continued upon the fire with the valve shut, because we may conclude that the heat was entering nearly at the same rate during the whole time. It is plain, however, that the experiment is not of such a kind as to admit of nice calculation; but it is abundantly sufficient to shew that a prodigious quantity of heat had escaped along with the particles of vapour produced from an inch of water. The water that remained could not be hotter than the boiling point, nor could the vessel be hotter, otherwise it would have heated the water, and converted it into vapour. The heat, therefore, did not escape along with the vapour, but *in* it, probably united to every particle, as one of the ingredients of its vaporous constitution. And as ice, united with a certain quantity of heat, is water, so water united with another quantity of heat, is steam or vapour.

There are some other instances of the same kind, which may perhaps appear still more striking, I mean the experiment made upon the boiling of fluids *in vacuo*, which are easily explained upon these principles, and serve at the same time to support them. The first person who made experiments of this kind, was the Honorable Mr. Boyle. He placed phials with hot water, which had been well boiled to expel its air, under the receiver of his new invented air-pump, and then exhausted the air. Long before the vacuum was nearly perfect, the water began to boil, and continued to do so with great voilence while he worked the pump. He expresses his surprise at the great heat which the receiver acquired, and at the sudden diminution of the heat of the water, which he expected to find still scalding hot when he admitted the air and took off the receiver, whereas it was only lukewarm. This experiment has been repeated by others many hundreds of times, and with more particular attention to the different circumstances. They all agree in this, that the water, from being hot, becomes lukewarm immediately, or very fast, and plainly in consequence of its boiling or producing vapour.

At the time that I was engaged in these experiments, professor Robinson, then one of my pupils, told me, that in some experiments which he had made with the air-pump belonging to the university of Glasgow, however hot the water was at first, the heat of it was rapidly diminished, till it fell to the 90th or 91st degree of Fahrenheit. Mr. Watt also assured me, that, in a very good vacuum, the water will continue to boil or produce copious vapour, till its heat be quickly reduced to the 70th degree of Fahrenheit. This sudden disappearance, therefore, of a great part of the heat of the water, while a very small part only assumes the vaporous form, is an additional proof of what I have advanced concerning the manner in which heat produces vapour.

Similar to these examples, are also, in my opinion, some surprising experiments made by Dr. Cullen, with some very volatile fluids, and published by him in the Physical and Literary Essays of Edinburgh. The Doctor employed the late

Dr. Dobson, at that time his pupil, to make experiments on
the heat or cold produced by mixing different fluids and solids
with one another. Dr. Dobson, in making these experi-
ments, observed that the thermometer, when lifted out of
many of the fluids, and suspended a short time in the air
beside them, fell down to a lower degree than that indicated
by another thermometer which hung constantly in the same
air. And, after varying his observations on this phenomenon,
he found reason to conclude, that it was occasioned by the
evaporation of the last drop of fluid which adhered to the
bulb of the thermometer; the sinking of the thermometer
being always greatest, and most quickly produced, when this
instrument was taken out of the most volatile fluids. Dr.
Cullen had then a curiosity to try whether the same phenom-
enon would appear on repeating these experiments under the
exhausted receiver of the air pump; and to satisfy himself,
he placed on the plate of the air pump, a glass goblet con-
taining water, and in the water he placed a wide mouthed phial,
containing vitriolic æther. The whole was covered with an air-
pump receiver, having, at the upper end, a collar of leathers,
in a brass socket, through which a thick smooth wire could be
moved, and from the lower end of this wire, projecting into
the receiver, was suspended a thermometer. By pushing
down the wire, the thermometer could be dipped into the
æther; by drawing it up, it could be taken out and suspended
over the phial.

The apparatus being thus adjusted, the air-pump was work-
ed to extract the air. An unexpected phenomenon immedi-
ately appeared, which prevented the experiment being made
in the manner intended. The æther was thrown into a vio-
lent agitation, which Dr. Cullen imputed to the extrication of
a great quantity of air. It is evident, however, from several
particulars, that the æther was not emitting air principally, but
a great quantity of æthereal vapour, and that it was truly in a
boiling state: for the elastic fluid, extricated by working the
pump, had a very strong odour of æther, and the quantity of
the æther in the phial, was considerably and suddenly dimin-
ished, while this boiling continued. And, what is especially

to be attended to at present, the æther became suddenly so cold, that it froze the water in the goblet around it, although the temperature of the air, and all the materials, were at the 54th degree of Fahrenheit, in the beginning of the experiment.

You will now easily understand how the phenomenon may be explained, by the principles which I have already establish-ed. It is evident, that the vitriolic æther is a fluid of such great volatility, that, were it not compressed by the weight of the atmosphere, which forces the parts of it to remain together, it would for ever be in the form of vapour, in all the variations of heat or cold, which we commonly experience; that the proper vaporific heat of this fluid, or the lowest degree at which it would boil, were it totally free from the pressure of the atmosphere, is far below the freezing point of water. It is therefore always more than hot enough to boil and produce vapour, whenever we take off the pressure of the atmosphere from its surface. Accordingly, when we take off this pres-sure from it, vapour begins immediately to be produced, and such a great quantity of the sensible heat that was in the æther is suddenly changed into latent heat, or absorbed in the va-pour, that the remaining æther must necessarily become ex-cessively cold. It may become so cold at last, as to be no longer in a condition to produce more vapour even *in vacuo*.

This experiment, therefore, has a good deal of analogy with the production of cold, by mixing snow with different salts. In those experiments, the snow is made suddenly to assume the form of a fluid, without adding heat to it. In the above experiment, the æther is made suddenly to assume the form of vapour, without its receiving any addition of heat.

We may further remark, that the above supposition, with regard to the lowness of the proper vaporific point of the vi-triolic æther, is not assumed without good reason: it is sup-ported by the analogy of the proper vaporific heats of other fluids. All of these which have hitherto been tried begin in a vacuum to boil, and emit vapour, when their heat is lower, by 120 degrees at least, than their vaporific point under the pressure of the atmosphere. But the vitriolic æther *is* well

known to boil under the pressure of the atmosphere at about
100 degrees of heat.   We have reason therefore to conclude
that it will boil *in vacuo* so long as its heat is higher than
the 20th degree below the beginning of Fahrenheit's scale,
or the 52d degree below the cold sufficient for the freezing of
water.

The more carefully we consider the production of expan-
sive vapour, in all that variety of circumstances that have been
already narrated and many others of the same nature, the more
satisfactory evidence have we, that a great quantity of sensible
heat is somehow absorbed by the water, in passing from the
liquid to the vaporous form, or disappears in some more un-
accountable manner during its formation.   For it does not
immediately appear in the steam.   This is evident by placing
the bulb of a thermometer in the way of the steam, so as to be
completely surrounded by it.   It raises the liquor in the stem
of the thermometer no higher than it would be raised by
plunging it in the boiling water.   As a farther proof that it is no
hotter, it is a matter of daily experience, that the steam of boil-
ing water does not scorch the most delicate flowers, nor spoil, in
the smallest degree, the fragrance or delicacy of their odorous
principle.   On the contrary, we obtain this principle in its
greatest perfection, by placing those flowers in the upper part
of a common still, in which water is made to boil.   The heat
of the passing steam volatilizes, and carries off along with it
the precious aroma ; and it is found in the water condensed
by the worm plunged in a refrigeratory filled with cold water.
This process is found to be vastly preferable (for such plants
as are uncommonly delicate) to the immersing them in the
boiling water.   It is plain, therefore, that the great quantity
of the heating principle, which we suppose to be flowing con-
tinually from the fuel in the furnace into the water contained
in the still, is not to be found in the steam, in the same active
form, or as the cause of heat.   But it is not lost, and is really
contained in the steam, in such a manner as to manifest itself
by its former effects, by heating any body when the vapour is
again converted into water.   As a particle of water, in the in-
stant of its becoming a particle of vapour, attracts and unites

with itself one or more atoms of this cause of heat, if only
raised to the requisite temperature, and retains them as parts
or ingredients of its vaporous form, so we may reasonbly con-
jecture, that when a particle of vapour again becomes water,
these atoms of heat are set at liberty by the fixed laws of
chemical affinity, and are then at liberty to enter into any other
body which is in a fit condition for receiving them.   Such a
body should be warmed by them, and this should be sensible
by means of the thermometer.   If indeed the body be on the
verge of liquefaction or ebullition, it may be as much disposed
to absorption of heat as the actually collapsing vapour of
water is disposed to its emission.   Yet, in this case also, we
should obtain equally incontrovertible indications of the ex-
trication of heat from the elastic steam.   The temperature of
the body may not be raised, but it will melt or boil.

When this opinion got hold of my mind, I was peculiarly
pleased with the explanation which it gave me of many curi-
ous facts observed in distillations, and other chemical pro-
cesses, which greatly perplexed the chemists, who being igno-
rant of their cause, could neither explain them, nor avoid
them when hurtful.   The multitude of important consequences
which seemed to result from this combination of heat with the
volatilized body, made me anxious to obtain accurate informa-
tion ; and the road that I was to follow seemed to be very ob-
vious and easy.   The object was to obtain, not only certain
proof of the extrication of a great quantity of heat contained
in the vapour, and not perceivable in it by means of the ther-
mometer, but also to discover whether all that heat was to be
obtained from vapour while condensing, which had disappear-
ed during its formation.   The proposed analysis was perfec'
similar to what I had accomplished, so much to my sati
tion, in the congelation of liquids compared with the l
faction of the frozen mass : and the mode of proceedin
equally obvious and familiar : I had only to attend to
happens in the working of a common still.

All of you know well enough how the operation of
mon distilling is conducted.   The water, or other liqu
be distilled, is put into a boiler, which is set on the fire.

vapour produced by its ebullition is not allowed to rise and
mix with the air, but is confined by the head of the boiler,
called the still, and it is conducted laterally by a pipe. But if
nothing more were done, the vapour would only blow out
through this pipe, as we observe it to do from the spout of a
tea-kettle, when it boils with violence. This pipe is, there-
fore, inserted into another, which is made to pass through a
large vessel, called the Refrigeratory, or Cooler, but without
communicating with it. The refrigeratory is filled with cold
water. This, being in contact with every part of the pipe,
keeps it cold, and the pipe being made of metal, which is
known to transmit heat very fast, the inside of it is quickly
robbed of any heat which the vapour can communicate to it;
and this heat is transmitted to the water in the refrigeratory,
and must raise its temperature. That this may be more copi-
ously done, the pipe is generally made of a spiral form, like a
cork-screw, its upper end communicating with the pipe from
the still-head, and its lower end being turned sidewise, and
led through the side of the refrigeratory, and a vessel is set
below its mouth, to receive the liquor formed of the condens-
ed vapour.

From this brief description of the apparatus (of which
more particular notice will be taken in its proper place) you
will easily comprehend the whole of the process, and the cir-
cumstance to which our particular attention must be directed.
The heat of the fuel penetrates the body of the still, raises the
temperature of the water (which I shall suppose to be the
substance under examination) till it can no longer remain in

liquid form, but boils, that is, it is converted into elastic
vapour; and, if my opinion be just, it absorbs, in the instant
of the version, or is converted by absorbing, a prodigious
tinuity of heat, namely, as much as would raise its tempera-
in the about 800 degrees, if it acted on it in the same manner
formore its boiling. I imagine that this heat is contained in
contaduced vapour, and goes off with it through the pipe.
by its its passage along the convolutions of the spiral pipe,
againm, as it is called, the hot vapour comes in contact with
stanircumference of the cold pipe. The fact is, that it is

completely condensed by this contact.   For, when things
are properly conducted, by giving a very extensive surface
of contact, and keeping a plentiful supply of cold water in the
refrigeratory, nothing issues from the mouth of the pipe but cold
water, and the air which occupied the upper part of the appa-
ratus, in the beginning of the distillation, and a small quantity
which is contained in almost all waters, and is extricated from
them by boiling.   In the mean time, it is a fact, that the
water in the refrigeratory grows warmer and warmer as the
distillation goes on, and indeed soon grows extremely hot, if
it be not allowed to run off, and its place be not supplied by a
continued stream of cold water.   When this is not carefully
secured, the liquor which drops from the worm is warmer and
warmer, in the same proportion as the water in the refrigera-
tory (for it evidently cannot be colder, but must rather be
somewhat warmer), and at last it is almost boiling hot.   Now
the condensation ceases, and steam blows through the worm
in abundance.   This has sometimes happened in the distilla-
tion of spirituous liquors, and has occasioned fatal accidents,
because these vapours are inflammable.

Such are the facts.   My notion of the internal procedure
was this: the particles of vapour which come into immediate
contact with the circumference of the spiral pipe, having
a smaller attraction for the heat combined with them, than
those of the cold pipe have, allow them to unite with the sub-
stance of the pipe, whence they are soon transmitted to the sur-
rounding water, so that the pipe is still in a condition to rob the
subsequent particles of vapour of their natural heat also. The
particles of vapour, deprived of their latent heat, are no longer
vapour, but water, which falls to the lower side of the worm, and
trickles down along it.   The exterior particles of vapour being
thus removed out of the way, those which were nearer to the
middle, or axis of the pipe, being pushed forward by the
steam behind them, and, at the same time expanding, come,
in their turn, to be applied to the sides of the pipe, and are
also condensed into water ; and thus, the different strata of
vaporous particles, from the circumference to the axis, come,
in turn, to be deprived of their heat, and, by the deprivation,

become water again, cooled down to the temperature of that part of the pipe by which they are condensed. These strata of vapour may be conceived as like the sliding tubes of a pocket spy-glass; those next the axis always getting beyond the stratum which surrounded them, before they expand and are condensed by touching the sides. By this process, the upper parts of the worm, and the surrounding water, become soonest hot, and are always hotter than those farther down the refrigeratory (if the water be not stirred). It is also reasonable to suppose that those parts of the pipe which are coldest condense the passing vapours most copiously, and that, when heated very near to the temperature of boiling water, they condense it very slowly indeed. Therefore more uncondensed vapour passes forward, to a part not yet so warm, where it is condensed, and by giving out its heat, that part of the pipe is more quickly heated; and thus the heating of the pipe and water proceeds to the bottom of the refrigeratory.

If this conception of the process be just, it should follow that no heat should disappear in the experiment, except what is unavoidably lost by warming the vessels, and, by their intervention, warming the surrounding air, and being carried off by it. All the heat transmitted from the fuel should be found in the distilled water, and in the water of the refrigeratory, except the loss by waste now mentioned.

Thus were the steps of my examination plainly pointed out to me. I must first measure the heat expended in boiling off a certain quantity of water. I must, in the second place, measure the heat communicated to the distilled water, and to the water of the refrigeratory; and I must compare those quantities together, and find them equal, or unequal.

When, for example, we set a still to work, the refrigeratory of which contains 100 gallons of water, of the temperature 50°, and we distil off one gallon; if we now examine the water of the refrigeratory, and find that it has gained 11 degrees of heat, we must reason in this way: had the steam, while it heated this water, produced no more effect than in proportion to its perceptible heat, and the quantity of water

which composed it, which is one gallon, it should not have increased the heat of the boiling water more than $1\frac{1}{2}$ degree. For it is little more than 150 degrees perceptibly hotter (the distilled water having the temperature 61° nearly). But it has actually raised the temperature of 100 gallons 11 degrees. Here, therefore, are $9\frac{1}{2}$ degrees of heat added to each of 100 gallons of cold water, without our being able to account for it according to what has been the common opinion. But a quantity of heat which is sufficient for increasing the sensible heat of 100 gallons of water by $9\frac{1}{2}$ degrees, could it be all put into one gallon, without converting it into vapour, would be sufficient for increasing the sensible heat of that gallon, by 950 degrees. And all this heat was actually contained in the one gallon, whilst in the form of vapour, and without shew- ing itself by affecting the thermometer more than boiling water does.

I made several experiments, in conformity to those already mentioned, on the conversion of water into steam, which fully satisfied my own mind that my opinion concerning the nature of elastic vapour was just. Indeed, when my mind was occupied by this thought, conviction flashed on me from every quarter, that the quantity af heat in all vapours was prodigiously greater than what was indicated merely by their sensible heats or temperatures. Every person knows the scald- ing power of steam, and that a momentary puff of it from the spout of a tea-kettle, which will scarcely render the hand damp, and does not contain the fourth part of one drop of water, will in an instant raise the whole hand into a blister, which a thousand drops of boiling water could not do. There is hardly a person who has not been struck by the great heat produced in the refrigeratory of a common still ; and those who manufacture spirituous liquors for the market have often found as great difficulty and expence attending the supply of cold water for their refrigeratory, as they had in supplying their furnace with fuel. This has prevented the erection of such manufactures in great cities, where it would otherwise be most convenient and profitable; because they cannot be supplied with cold water. The more I reflected on these

things, I was the more astonished that a thing so obvious, and so interesting to a number of active people, attentive to their interest, and accustomed to estimate every thing by measurement of some kind, had never attracted their attention, and engaged them, or speculative men, who are always ready to assist them, in some comparison between the fuel expended and the remarkable heats produced. It now appeared wonderful to me, that a competent knowledge of this subject was not acquired almost as soon as the scale of the thermometer was determined by the fixed points of boiling and freezing water. And, when that noble machine the steam engine, was invented, and was continually in the hands of most ingenious men, such as Dr. Hooke, Dr. Papin, Dr. Desaguilliers, and others, and almost prescribed this examination as indispensably necessary for its very practicability, it is still more surprising, that so obvious a thing never occurred. But no such examination is to be met with in the writings of either the mechanicians or the chemists.

The latent heat of steam seemed to me to be so completely established, that I was little solicitous of more experiments for my own conviction, or for making the doctrine clearly comprehensible to others. I, therefore, taught it in my annual courses, and have reason to believe that it was perfecly understood by my students. But it was a desirable thing to obtain exact measures of the heat contained in steam, because this appeared to me the most accurate way for obtaining measures of the heat produced by fuel, and might be of great service in making trials of the most economical methods of applying fuel, a point of immense consequence in ninetenths of all our manufactures. It was some considerable time, however, before I could execute this to my satisfaction, owing to the occupations of a double academical duty, and the attention due to my patients. And, at last, indisposition obliged me to commit the conduct of the experiments to one of my pupils, the late Dr. Irvin of Glasgow, a young gentleman of an inquisitive and philosophical mind, of great ingenuity, and peculiarly qualified for this task, by the habits of mathematical study, and scrupulous attention to all kinds

of measurement. The following was the first of the experi-
ments for this purpose, which I made in Glasgow, with the
assistance of Mr. Irvin, on the 9th of October 1764.

Five measures (each containing 4 lb. 5 oz. and 6 dr. avoir-
dupois) of water, of the temperature 52°, were poured into a
small still in the laboratory. The fire had been kindled about
40 minutes before, and was come to a clear and uniform state.
The still was set into the furnace, and, in an hour and twenty
minutes, the first drop came from the worm; and in three
hours and forty-five minutes more, three measures of water
were distilled, and the experiment ended. The refrigeratory
contained 38 measures of water, of which the temperature, at
the beginniug of the experiment, was 52°. When one mea-
sure had come over, the water in the refrigeratory was at 76°.
When two had come over, it was at 100°; and when three
had come over, it was at 123°.

In this experiment, the heat, which emerged from three
measures of water in the refrigeratory from 52° to 123°, or 71°.
Now 3 is to 38 as 71 to $899\frac{1}{3}$, and the heat would have raised
the three measures $899\frac{1}{3}$ degrees in its temperature, if this
had been possible without converting it into vapour. The
heat of the vapour from which this emerged, was 212°, or 160°
more than that of the water. Taking this from 899°, there
remains 736°, the heat contained in the vapour in a latent
state.

But this must be sensibly less than the truth. During the
experiment, the vessels were very warm....the head of the still
as hot as boiling water, and the refrigeratory gradually rising
from 52°, which was within a degree or two of the tempera-
ture of the air of the laboratory, to 123°. A very consider-
able portion of the latent of the steam must have been car-
ried off by the air in contact with a considerable surface, some
of which was exceedingly hot. A great deal must also have
been carried of in the steam which arose very sensibly from
the water in the refrigeratory, towards the end of the experi-
ment. Mr. Irvin also observed, that, during the distillation,
the temperature of the water which ran from the worm, was
about 11° hotter than the water in the refrigeratory. The

steam, therefore, at a medium, was not 160° hotter than the water which ran from the worm, but 125°, its heat temperature being about 87°. This consideration alone will make the latent heat of the steam not less than 774 degrees, without any allowance for waste.

Some comparison may also be made between the heat expended in the production of the steam, and that which emerges during its condensation. The time which elapsed during the raising of the temperature of the five measures of water from 52° to 212°, that is 160°, was one hour and twenty minutes, or 80,....and 225′ elapsed during the boiling off of three measures. Therefore, since 80 is to 225 as 160 to 450, as much heat was expended as would have raised the five measures 450° in temperature. This would have raised three measures 750° above the boiling heat already produced. This gives 750 for the latent heat of the steam, besides what was unavoidably lost by communication to the ambient air, and what was expended in heating the vessels.

This is an outline of the experiments, which must be made for ascertaining this fundamental point. A few weeks after, Mr. Watt made similar experiments with a smaller still, better fitted for a trial of this question, and much more manageable. The medium result of these trials gave 825° for the heat contained in the steam. I have narrated the procedure, that you may see the circumstances which must be attended to, in order to draw a conclusion from them. What I have related is sufficient for establishing the main principle, namely, that the heat which disappears, in the conversion of water into vapour, is not lost, but is retained by the vapour, and is indicated by its expansive form, although it does not effect the thermometer. This heat emerges again from this vapour when it becomes water, and recovers its former quality of affecting the thermometer ; in short, it appears again as the cause of heat and expansion. But much remains to be done, before the experiment can be made use of for giving us a precise measure, either of the heat imparted from the fuel, or of that which emerges from the steam. A train of experiments must be made for obtaining the precise rate at which

the heat of the fuel passes into the water during the rising of its temperature, in order that from thence we may conclude the rate at which it continues to enter while the water is distilling. Another train of experiments is necessary for ascertaining the whole loss of heat by the vessels, and by communication to the air. To relate these would consume much time, and they will readily occur to any person much conversant with chemical processes.

I think it sufficient to inform you, that Mr. Watt, in the course of his studies on the steam-engine, has made all the necessary experiments with the most scrupulous care, knowing that the improvement of that noble engine must depend entirely on an exact knowledge of the procedure of nature in the formation and condensation of steam. Mr. Watt informs me, that he has observed as exact coincidence between the heat rendered latent in the vapour, and that which emerges from it, as can be desired; and that the heat obtainable from steam, capable of sustaining the ordinary pressure of the atmosphere, is not less than 900 degrees of Fahrenheit's scale, and that it does not exceed 950.

But this is not the only way in which it may be measured; there being other indications of the presence of heat, that are equally susceptible of measure. I have already observed, that if the body employed for condensing the steam be in an absorbing state, its temperature will not be increased by what is thus absorbed. But, as this absorption and new combination of heat, is accompanied by a change in the constitution of the body, which is sufficiently remarkable, viz. the body being either melted or volatilized; and, as the quantity of matter thus changed in its form, must be proportional to the heat combined with it, it follows, that the heat which emerges from a given quantity of vapour, of the temperature 212°, will melt a precise quantity of ice, or any other body; and therefore, the quantity of heat latent in steam, may be compared with that contained in the boiling hot water from which it is produced, namely, by comparing the quantities of ice melted by both. This, accordingly, was one of the first methods which occurred to me for the measurement, and it came naturally into my thoughts.

as I had, not long before this, been employed in the analysis of liquefaction. I projected some experiments for this purpose to be tried in the ensuing frosts. But my friend Mr. Watt, being now warmly engaged in the same enquiry, which had become so interesting to him, I was soon supplied by him with measures abundantly accurate for my views. The cele-brated philosopher, Lavoisier, took this other method of measurement, and invented a most ingenious apparatus for the purpose of measuring all productions of heat, by the quantity of ice melted by those operations of nature in which it appears. The calculations deduced from such of his ex-periments as have come to my knowledge, gave a somewhat higher value of the latent heat of steam, making it 1000 de-grees, or perhaps a little more.

It is unnecessary to add any thing more to the preceding facts, to prove that the interruption in the calorific process, in the conversion of fluids into elastic vapour in the act of ebullition, is perfectly similar to that in the process of liquefaction ; and I flatter myself that we may now take it as a point fully es-tablished, that, *when a fluid body is raised to its boiling tem-perature, by the continual and copious application of heat, its particles suddenly attract to themselves a great quantity of heat and, by this combination, their mutual relation is so changed, that they no longer attract each other, gathering into drops and forming a liquid, but avoid each other, separating to at least ten times their former distance, (for a cubic inch of water forms much more than a thousand cubic inches of vapour), and would separate much farther, were they not compressed by the weight of the atmosphere ; and in short, they now compose a fluid, elastic, and expansive, like air.*

This new form of aggregation is the effect of a new combi-nation of heat with the primary particles of water, and is a sufficient indication of this union, in the same manner as fluidity was a sufficient mark of a sudden and copious combi-nation of heat with the particles of ice. We cannot, perhaps, form a very distinct notion of the nature of this combination, so as to understand its distinction from that which takes place in all lower temperatures, and is accompanied only by expan-

sion of the ice, without any change of form.   In the same
manner, we do not know the difference between the mode of
union, which takes place between heat and water in all tem-
peratures, from 32° to 212°, and that which obtains whenever
we attempt to increase this last degree of heat by a more
copious supply.   We must content ourselves with observing
the fact, and remembering that the form of elastic vapour is
the characteristic of a very peculiar combination of heat with
the elements of other bodies.

We had some difficulty in conceiving how 130 or 140 de-
grees of heat could combine with the particles of ice, with-
out increasing its bulk in the same proportion as it had done
in lower temperatures, and as it continues afterwards to do in
water; but, in the present case, we have no such difficulty,
because the bulk of the compound being much more increased,
than in the proportion of the preceding expansion*, there
seems to be more room than before, where it may be lodged
without redundancy.   We are not surprised, therefore, at
finding that the temperature is not increased; for, instead of
thinking that heat should flow out, and enter a thermometer
already expanded 212 degrees, we should rather expect, that
when the bulk has increased 1800 times, while only 900 de-
grees of heat are thrown into it, there would rather be a dis-
position in the heat to quit the thermometer, and enter into
this empty space.   I do not pretend to say that heat is con-
tained in vapour, or in water, as water is contained in the
pores of a sponge†, but only that there does not appear any

* Mr. Watt has made a great number of experiments, for ascertaining
the bulk into which a cubic inch of water is expanded when it composes the
steam, which, at the temperature 212°, sustains the pressure of the atmos-
phere; some of these were direct, by evaporating a known quantity of
water....others were more circuitous, deduced from the performance of his
engines.   The medium result gave about 1800 cubic inches.   We may say,
that a cubic inch of water forms a cubic foot of such steam.   EDITOR.

† There are phenomena, indeed, which have induced many naturalists of
reputation to think so.   Air, steam, and other elastic fluids, raise the ther-
mometer when suddenly and greatly compressed; but this inference is too
hasty, and a proper examination of the union between these substances and
heat, renders it extremely doubtful, whether this emission of heat should ne-
cessarily follow the compression.   EDITOR.

thing unaccountable in the fact, that this great accumulation of heat does not affect the thermometer. We now see, in some degree, how it cannot. The thermometer cannot rise without being hotter; but, if hotter, it cannot condense steam; and if the steam be not condensed, its heat cannot emerge, because whatever caused the absorption of heat maintains it absorbed. But, whatever be the nature of the equilibrium of temperature among bodies, it remains unaffected by the change which the water has undergone in its form. Whenever a body hotter than 212° comes in contact with the vapour it raises its temperature and expands it: and whenever a body colder than 212° touches it, the vaporific union is immediately dissolved; heat quits the vapour, and it becomes water. The heat emerges in all its intensity and abundance, if there be enough of colder matter to receive it, but not else. Therefore, although the steam from the mouth of a tea-kettle will not raise a thermometer above 212°, a single puff of it, which does not contain one grain of water, will scald a considerable extent of the skin, as much as if it were plunged for a moment in boiling water. The rapidity with which this transition of a great quantity of heat is made, is wonderful, when the surface of cold matter is sufficiently extensive. Two English gallons of ice-cold water, dashed in small drops through the capacity of a steam cylinder holding three hogsheads, will condense the vapour which fills it, in less than one-fourth of a second.

There are other ways, however, in which vapour may be condensed, without drawing off its latent heat by means of cold bodies. It may be condensed by mechanical compression, and in this case also we see its latent heat emerge.

This is a nice and troublesome experiment, and requires a very nice apparatus. A metal cylinder, nicely bored, is fitted with a piston; and the cylinder is made to terminate below, in a small glass tube, close at the end. A thermometer is so fixed to the cylinder, that its bulb is in the axis, and the tube projects ontwards through the cylinder. A little water is poured into it, so as to fill the glass tube at the bottom. The piston is put into the top, and a small hole in the piston is left

open. A large glass cylinder is filled with water, kept boil-
ing hot. The water in the glass tube is made to boil briskly,
the steam blowing through the hole in the piston. In this
state it is immersed in the boiling water, the hole in the pis-
ton speedily stopped, and the piston thrust down with great
force, at least half way. The thermometer rises imme-
diately, and the water in the glass tube is seen to increase in
bulk. The whole must be done with great despatch.

We now see clearly why the boiling point of a fluid is so
constant, and why no violence of fire can make water in an
open vessel hotter than 212 degrees. The instant that any
portion of it attains that temperature, it absorbs the heat as
fast as it can be transmitted through the vessel, and becomes
elastic vapour, which rises through the rest, and escapes. This
rapid absorption of heat is most distinctly perceived by means
of the little philosophical toy, called a pulse-glass, the ingeni-
ous contrivance of Dr. Franklin. This consists of two thin
glass balls, A and B, (fig. 2.) connected by a slender pipe.
One of the balls, and a small part of the pipe, is filled with
water; and this being made to boil violently, the steam blows
all the air out of the other ball, and follows it. By stick-
ing the end of the pipe C into a bit of tallow, the hole is stop-
ped up, and the steam in the ball B soon collapses, and
becomes water, so there is now a fine vacuum in this ball. The
flame of a blow-pipe being now directed on the neck of the
pipe C, it is soon hermetically sealed. Thus we have re-
moved the pressure of the atmosphere from the surface of the
water, and it will now boil by the heat of the hand, if the ball
B be kept a little colder, that it may condense the steam as
fast as it comes into it. It will be a long time, however, be-
fore the warmth of the hand heats all the water in A sufficiently
and it will not boil till this be done. But if we hold the end
B a little down, so that some water may run into it from A,
and thus leave a part of A empty, and then, holding the instru-
ment level, if we grasp A in the hand, the heat is sufficient
for immediately converting into vapour the thin film of water
which now lines the empty part of A. The vapour expands,
pushes all the water out of A into B, and then rushes up through

it, throwing it into the agitation called boiling, and it is a real boiling, although with a warmth that is imperceptible. On the contrary, the ball A feels even cool to the hand. If we allow all the water to come back again, except so as to have a very small bubble in the upper part of A, and now apply the tongue to that part, we shall immediately throw it into a violent ebullition in the ball B, and A will feel very cold to the tongue. The ebullition and the cold will continue till all the film of water which lined the inside of A is completely evaporated, and it has become perfectly dry. The boiling in B now ceases, and, with it, the coldness of A. Nothing can shew, in a more convincing manner, the prodigious quantity of heat absorbed by water, in the act of its conversion into vapour, than the coldness produced, during a very considerable space of time, in order to evaporate this trifling film of water.

It is this great quantity of heat contained in steam that makes it so powerful and effectual in the business of cookery. The knowledge now acquired of the latent heat of steam, has, of late years, been applied by ingenious artists to this purpose, with very great effect, giving to all the operations of the kitchen, where boiling or stewing is concerned, a neatness and easiness of management that is very remarkable; and this is attended by a great saving of fuel. Whoever attends to the manner in which heat is applied, in our kitchens, to all cooking vessels, will be convinced that not less than three-fourths of our fuel is wasted, in the production of a heat which is extremely troublesome to the cook, and, after all, goes up the chimney, without doing any service. A great surface of fuel, in a state of incandescence, heats a great quantity of air, both by radiation and by material communication. We set a boiler on this open fire, in the midst of a copious stream of heated air, which is incessantly rushing up the chimney. Only a very small quantity of this comes in contact with our boiler. The bottom of it is indeed, heated by the fuel also on which it rests; but the greatest part of all the heat produced is running to waste. If, instead of setting the vessel on the open fire, we set it on a well constructed furnace, where the heated air shall all be forced to come in contact with its bot-

tom and sides, we are certain that more than nine-tenths of the fuel may be employed in communicating heat to water boiling in the vessel, the remaining tenth being expended in maintaining the heat of the furnace, or escaping with the air by the flue. By this operation, the water is converted into elastic vapour, furnished with its latent heat, which is ready to emerge and affect any body that is colder than 212°. It would therefore, very quickly heat the water in another similar vessel up to the same temperature, and would keep it in that state, although it cannot cause it to boil. And it will do this in a manner far superior to the hot air of a fire; for this last acts only by the film, which is in perfect contact with the vessel; and the hot air, which is half an inch farther off, passes by, without doing the least service. But, if the steam from our boiler be admitted into a box, already heated to 212°, and containing a piece of meat, or a vessel which contains any thing to be dressed by a boiling heat, not a particle of vapour will pass by it, but will all come and deposit on it the whole of its latent heat. And this will continue without interruption, till the vessel, with its contents, have attained the temperature 212°. Then, and not till then, will the steam fill the box, and blow through any hole made in its top or sides. This hole may communicate with another box. The steam, *having nothing left it to do* in the first, will go into the second, where it will be equally effective. Steam is the most faithful carrier of heat that can be conceived, and will deposit it only on such bodies as are colder than boiling water.

In constructing an apparatus for this purpose, the most adviseable method seems to be, to convey the vapour from the boiler into a long wooden box, divided by partitions into portions fit for holding the different vessels of cookery, and always to employ those which are nearest to the boiler. If any thing be dressing in a remote division, and we put a cold vessel into an intermediate one, the operation in all the divisions beyond it will be suspended, till the new vessel be boiling hot; and the others will now be cooling, and we must expend steam in order to restore their proper heat. Wooden

boxes are recommended, because they will waste less heat by communication to the air; but they must be lined with thin metal, otherwise they will soon be destroyed by the steam.

Employed as we are just now, in the consideration of the production of elastic vapour, and its condensation into water again, it would be unpardonable to omit taking notice of the application which has been made of these properties to the arts of life, by the invention of the STEAM ENGINE; an invention, which, I may truly say, is, in its present state, the master-piece of human skill*. Nor was it, like the pump, the mariner's compass, the telescope, the production of a chance observation, but the result of deep thought and reflection, and really a present by philosophy to the arts.

The power of heat to increase the elasticity of steam to the most enormous degree, had been observed by many, and Dr. Papin had made an ingenious use of the heat accompanying its great compression in his digester. The Marquis of Worcester, a gentleman of extensive natural knowledge, and most inventive mind, observed farther, that steam, when exerting this enormous force, could be, in an instant, deprived of it, by dashing a little cold water through it, and that a vacuum would thus be produced in the vessel, which the moment before was ready to burst. He therefore, about the year 1660, contrived, most ingeniously, to make this vessel V (fig. 4.) communicate alternately with a boiler B, from which it should receive, through a cock S, this highly elastic steam, and with a cistern C, from which it was required to raise or force the water. The vessel being supposed to be filled at first with common air, the steam was admitted, and it quickly blew out all the air. The steam-cock being now shut, a jet of cold water was admitted by another cock I, which, in a moment, condensed the steam, and made a vacuum, and water rushed in from the cistern and filled the vessel. The condensing cock being now shut, the steam-cock was again opened; strong steam was admitted, which by its violent pressure on the surface of the water, drove it out by another pipe EF, which conveyed it to where it was wanted.

* See Note 35 at the end of the volume.

This most curious contrivance was immediately put in practice, and excited the wonder of every person in that most ardent and inquisitive age. The engine was found, however, so limited in its practicable powers, and so difficult, and even hazardous to manage, that it never came into very general use. But, about the beginning of this century, its principle and form underwent a great revolution. The great elasticity, which made it so hazardous, was found unnecessary; and the steam, no stronger than common air, was employed merely as a mean of rapidly making a vacuum in a vessel by its condensation. The contrivance was as follows:

The steam-vessel V, (fig. 5.) is now a metal cylinder, smoothly bored, communicating with the boiler B, through the steam-cock S, and with the cold water cistern A, through the injection-cock I. A great piston, P, exactly fitting the cylinder, hangs by one end of the working beam DH, which turns round an axis or gudgeon, G. From the other end of this beam, hangs the piston Q of the pump, which draws the water from the pit. This end of the beam is made to preponderate, so that, when the great piston P is at the bottom of the cylinder, the weight hanging on H shall be able to overcome all the frictions, and to drag the great piston to the top of the cylinder. The sketch, therefore, represents the engine in its natural inactive position.

The water being made to boil strongly in the boiler, the cock S is opened, and the steam rushes into the cylinder, and drives all the air out of it by the pipe E near the bottom. When the attendant observes the steam following in abundance, he shuts the steam-cock, and opens the injection-cock I. Cold water now spouts up into the cylinder, and plays against the piston P, and, scattering over the whole vessel, immediately condenses the steam. The pressure of the atmosphere shuts the valve lying on the mouth of the steam-pipe E; and a vacuum is now produced in the cylinder. The atmosphere presses on the upper surface of the great piston, with a force of nearly 15 pounds on every inch, or a ton on every square foot. This forces down the great piston P, and draws up the piston Q with the column of pit-water corres-

ponding to it.   When the piston has reached the bottom of
the cylinder, the attendant shuts the injection-cock, and opens
the steam-cock.   The steam, being as elastic as common air,
performs the same office that the opening of the valve E would
do, that is, it allows the preponderancy at H to drag the great
piston to the top of the cylinder, where it is again ready to re-
ceive the pressure of the atmosphere, and make a second
working stroke.

It is plain that this construction has totally changed the
engine, both in form and in principle.   We no longer need a
highly elastic steam, in order to raise a great column of
water.   We need only to enlarge the diameter of the cylinder,
till the pressure of the atmosphere supplies a force equal to
the task.   A cylinder of 30 inches diameter supplies a force
of five tons.   It is a vulgar mistake, that the prodigious
power of this engine is owing to a great force in the steam.
Its only use is the rapid production of a vacuum, by the con-
densation produced by the injection of cold water.

Such is Newcomen's engine.   By its assistance, many
mines have been continued, and many have been opened and
worked, with great advantage to the proprietors and to the
public, which must have otherwise been abandoned, notwith-
standing the earth was full of treasure ; and the abundance of
pit-coal procured, by these means, to these kingdoms, gave it a
superiority in many manufactures, which has been of un-
speakable advantage to this nation.

I have the pleasure of thinking, that the knowledge which
we have acquired concerning the nature of elastic vapour, in
consequence of my fortunate observation of what happens in
its formation and condensation, has contributed, in no incon-
siderable degree, to the public good, by suggesting to my
friend Mr. Watt of Birmingham, then of Glasgow, his im-
provements on this powerful engine.   The great hindrance
to its employment, for every purpose, not only of mining, but
even of manufactures, by giving us a moving power of un-
limited force, has been the prodigious expence of fuel.   An
engine, having a cylinder of three feet diameter, and working
continually, consumes about 2000 chaldrons (London measure)

of coals in a year, or almost 3000 tons*. Many attempts have been made by engineers to diminish this enormous expence ; but no way occurred to them but improving the construction of their furnaces, and by preventing all waste of heat that could be avoided. The flame was made to circulate round the boilers ; the fire was placed in the middle of the boiler ; the machine was carefully defended from the air, in all places where its heat was great. Mr. Watt also directed his attention to these things ; but he soon found that all these were but trifling improvements, and that the copious production of steam was not enough, unless it was afterwards husbanded with the most rigid economy. He saw that more heat was wasted after the steam was produced, in consequence of its being misman-aged, than by the most injudicious construction of furnaces. For, as steam contains such a great quantity of heat, a waste of steam is a sure waste of heat. As much heat is contained in one gallon of water in the form of steam, as would bring $5\frac{1}{2}$ gallons of ice-cold water to a boiling heat.

In Newcomen's engine, a vast quantity of steam is wasted by unavoidable condensation, the moment it enters the cylinder ; for the cylinder has been cooled down to 100°, or 110°, by the injection ; and there is lying at the bottom a quantity of water, perhaps even colder than this. It is also cooled by the descent of the piston, covered with several inches of cold water. All this must be heated again to the temperature 212°, before the piston begin to rise ; and, as it goes up, more of the cooled surface of the cylinder is laid open to the steam, and condenses it ; and the piston cannot reach the top till the whole cylinder be again raised to 212°. Then comes the injection, which again cools every thing. It is plain that this waste will be increased by injecting too much cold water ; therefore the engineers were careful not to do this. But they blun-dered as much by injecting too little ; for this quantity of

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* 100 pounds weight of the best Newcastle coal, when applied by the most judiciously constructed furnace, will convert about $1\frac{1}{2}$ wine hogsheads of water into steam, that supports the pressure of the atmosphere.

water, not being sufficient to absorb all the latent heat of
the steam, was raised to a considerable temperature, and
therefore allowed a steam to remain, whose elasticity bore
a very sensible proportion to the pressure of the atmos-
phere; and this balanced that portion of the pressure, and
rendered the piston unable for the full load of 15 pounds
on every square inch. To preserve this power, requires
the injection of an ale pint of cold water, for every cubic
inch of water that has been converted into steam, or for
every cubic foot of steam condensed. Mr. Watt imagined
that half of the steam produced in the boiler was thus
wasted.

But how was this to be remedied? The waste is una-
voidable, while the steam-vessel is thus cooled by every
injection; therefore it must be wasted as long as the con-
densation is performed in the cylinder, or steam-vessel.
He therefore contrived to perform it in another vessel, in
the same way as it is done in the common process of dis-
tillation. His condenser is a separate vessel, occasionally
communicating with the great cylinder. He knew that
steam, being an elastic fluid, will go into any communi-
cating vessel, if emptied of its air, or other contents.

And if this condensing vessel be kept extremely cold, by
being immersed in cold water, or any other method, he
presumed that it would all be condensed there. But all
this is not enough. The cylinder will still be cooled by
the descent of the piston, and the cold water above it. Mr.
Watt, therefore, did not allow the external air to press
down the piston, but employed the hot steam from the
boiler, which is equally powerful, its elasticity being already
a balance for the pressure of the atmosphere: it will,
therefore, press as strongly on the upper surface of the
piston. Thus is the cylinder kept continually hot,....as
hot as boiling water; and it may be inclosed in a case, to
prevent loss of heat from so hot a surface of great extent.

Mr. Watt's construction is, therefore, as follows: the
steam-vessel D (fig. 6.) communicates with the boiler F,
by the pipe A A leading from its top, and having a stop-
cock A. It communicates at the bottom with the conden-
ser E, which is immersed in a cistern of cold water. The

pipe of communication has a stop-cock B.   A communi-
cation is also established between the upper and under
parts of the steam vessel, by means of the pipe C C, also
furnished with a stop-cock C.   Both D and E are neatly
bored, and have pistons, which move smoothly up and
down, without allowing any air, or other fluid, to pass by
their sides. Both pistons are furnished with shanks, which
move air-tight, in holes made in the covers of the cylin-
ders.   The quiescent position of the engine is such, that
the pistons are at the top of the cylinders D and E, be-
cause they hang from the inner end of the great beam,
and the outer end of it is made to preponderate a little, so
as to drag both pistons to the top of the cylinders.

   In setting the engine to work, all the cocks are opened.
The steam, passing through the cock A, gets into the top
of the cylinder, and, from thence, along the pipe C, into
the bottom of the cylinder, and fills it, driving out the air
through the cock B into the condenser, and then through
the valve of the condenser-piston.   This being done, the
cock C is shut, and now, as the steam is condensing rapidly
in the condenser, it is rarefied in the cylinder below the
piston, while it presses on the upper surface of it with its
full force.   It therefore presses down the piston, almost
to the bottom, but not altogether, by reason of some air
remaining in it.   When the piston is at the bottom of the
cylinder, the cocks A and B are shut, and C is opened.
Thus, by establishing the communication between the top
and bottom of the cylinder, the steam presses equally on
the upper and under surfaces of the piston, and now the
preponderancy of the outer end of the beam is permitted to
drag the piston to the top again ; and the steam, which
was formerly all above it, is now all below it, having come
round by the cock C.   This cock is now shut, and A and
B are opened.   There is, at present, a vacuum in the re-
frigeratory E, by the rise of its piston. The steam, there-
fore, in the cylinder expands into this empty space, and,
as fast as it enters, it is condensed by its cold sides, and
falls to the bottom, in the form of water.   A vacuum
being thus made below the piston, it is pressed down by
the steam, even although this steam were no stronger than

common air. It presses down with a force equal to the
sum of the pressure of the atmosphere, and the excess of
its elasticity above that pressure. The water produced in
the refrigeratory, by the condensation of the steam, and
the small quantity of air which is extricated from it, or
has entered by crevices, or remained after the first descent
of the piston, is extracted at the next upward movement
of the piston of the condenser.

By this construction, the cylinder is kept boiling hot, and
wastes no steam. The vacuum also, made by the conden-
sation, may be as perfect as we please, because there is no
occasion to be sparing of cold water for the condensation.
This circumstance gives a great mechanical superiority to
this construction. For it has been found by long expe-
rience, that in the old engine, it is adviseable to work
against no greater load than six or seven pounds on every
square inch of the piston, because the quantity of cold water
that must be employed, for producing a greater unbalanced
pressure on the piston, cools the cylinder so much, that
more loss is sustained by the expence of fuel to replace the
wasted steam, that is gained by the increase of power. In
Mr. Watt's construction, the engine will work very tolera-
bly against a load of $13\frac{1}{2}$ pounds, and is never loaded with
less than $11\frac{1}{2}$; but its most conspicuous advantage is, that
an engine of this construction consumes not more, and
generally a good deal less, than one-third of the fuel used
in the very best common engine of the same size.

Mr. Watt still farther improved this engine, by making
it exert an active stroke in both directions. This was very
obvious and easy. He had only to make both the upper
and under parts of the cylinder communicate with the boiler
and condenser. When the upper part communicates
with the boiler, and the lower with the condenser, the
piston is strongly pressed down ; and when these commu-
nications are reversed, it is as strongly pressed up. More-
over, by an artful cutting off, or greatly diminishing the
supply of steam, before the piston has completed its move-
ment, he has enabled a given quantity of steam to produce
a much greater effect, and at the same time has brought
the engine so much, and so instantaneously, under com-

mand, that it is really more manageable than the exertion of any animal. But the explanation of these particulars is not in our department, but that of the mechanician.

I must not, however, omit an important chemical obser-vation which occurred to Mr. Watt in the course of those improvements, and particularly in the prosecution of a pro-ject which he had formed for distilling *in vacuo.* This is a project which I have thought of ever since I saw the curious experiments of Dr. Cullen, already related to you.*
In these it was plain, that when the pressure of the air is removed, volatile liquors will boil, and therefore distil, at very low heats. Mr. Robison's experiments, already men-tioned, also shew that water will certainly distil at the tem-perature 90°, and vinous spirits at the temperature 55°. I see that Fontana has had the same project; and indeed it is very obvious. Mr. Watt, having carefully examined the elasticity of watery vapours, in a variety of tempera-tures, between 32° and 212°, concluded that water would pass copiously by distillation, when of the temperature 70°, if the refrigeratory be kept in melting snow ; or, that it will distil equally well of the temperature 90° or 95°, in the ordinary temperature of this climate.

He accordingly made the experiment, and it succeeded perfectly well. A very small still was half filled with water, and then closely united with the vessel which was to re-ceive the distilled water. A very small hole was made in the bottom of the receiver, and a plug was fitted to it. The water being made to boil violently, sent the steam through the whole apparatus, (there being no water in the refrige-ratory to condense it) forcing the air out by the hole. While the whole was boiling hot, and the steam blowing through the hole, this was suddenly stopped up by the plug, and the bottom of the still was set on ice. This soon cooled its contents ; and the steam, which occupied the rest of the apparatus, collapsed into a few drops of water. A lamp was now set under the still, and in a few minutes the whole apparatus grew warm : a proof that steam was now produced from the water, and passed over into the

* I see this project in a note-book written before 1757.

receiver.   Cold water was put into the refrigeratory, and
the distillation went on ; slowly indeed, but very well ;
and the ebullition was very distinctly heard and felt in
the still, although the head of it was scarcely warm to the
hand.   But the result of the process was, that the latent
heat of the steam was greatly increased by this diminution
of its sensible heat.   The temperature of the steam in the
experiment was found to be 100.   The water in the refri-
geratory was raised by the condensation of the steam, from
57° to 77°, and the vessel had acquired as much heat as
could have raised the water one degree.   Therefore, 21
degrees of heat had been acquired from the steam.   The
quantity of water distilled was $\frac{1}{51}$ of that in the refrigera-
tory ; therefore, the increase of temperature, namely 21°,
multiplied by 51, will give the heat extricated from the
steam.   This is 1071.   From this take 100-77, or 23, the
sensible heat lost by the steam, and there remains 1048 for
the latent heat of this steam.

Mr. Watt has made other experiments, with much more
care, and in various states of heat and elasticity of the
steam.   He finds that water distils perfectly well, when of
the temperature 70°, and that, in this state, the latent heat
of the steam approaches to 1300, and certainly exceeds
1200.   The unexpected result of these experiments is,
that there is no advantage to be expected, in the manu-
facture of ardent spirits by distilling *in vacuo*.   For we
find that the latent heat of the steam is at least as much
increased as the sensible heat is diminished.   This will
undoubtedly be attended with an increased expenditure of
fuel.   For the increase of 100 degrees of sensible heat oc-
casions an increase of fuel, only while we are raising the
temperature of the still to the ordinary heat of boiling
water, in the beginning of the distillation.   If the furnace
be judiciously constructed, and due precautions taken to
prevent dissipation, it requires very little fuel to maintain
this temperature.   But 100 degrees of latent heat is an ex-
pence that is continual, and which no contrivance whatever
can prevent.

This train of experiments suggested to Mr. Watt many
very curious and important reflections concerning the com-

binations of bodies with heat ; and his opinions on this subject have been farther established, by the whole course of his experience, in the construction and management of his double-stroke steam engine.

We are now much more able to understand the nature of vapour than formerly, and to trace some of the steps by which it is produced.   It is surely an approach to the explanation of any phenomenon in nature, when we obtain certain information of circumstances indispensably necessary for its production ; and the clearness or competency of this explanation is greatly augmented, when those circumstances are of a measurable kind, and invariably connected with certain gradations in the phenomenon itself. We seem to have attained this desirable point in the present case, and in that of liquefaction and congelation. There is a something to which I have given the familiar name of heat, which is susceptible of degrees.   It not only gives us the sensation of greater and less warmth, or heat, but it also produces other effects, which are equally variable and distinguishable in quantity.   We might take any of these variations for the measures of its degrees, if we find them always consistent.   We have not been able to measure our feelings of heat with accuracy, and we even observe, that the most accurate measures of them would not be accurate measures of their cause ; because, in cases where we know that the general cause is the same, the degree of the sensation may be different.   The expansion of certain bodies has been found a very good measure, merely because, in cases in which we know, *by other proofs*, that the cause is the same, or double, or triple, the expansion is also the same, or double, or triple.   But, in the application of this measure to our purposes, we met with two very remarkable irregularities, which, had they occurred earlier, might have hindered us from adopting expansion as a measure of heat, and deprived us of the thermometer,....I mean the passage of a body from solidity to a liquid form, and from this to the form of vapour. Had we only examined bodies which change their state gradually, such as sealing wax, we should perhaps have formed very different notions of the scale of heat, after

which we should have been in continual mistakes, while considering the heats or temperatures of such bodies as water, lead, mercury, &c. But the observations I have laid before you have removed all those difficulties, and have enabled us to include even those great anomalies in our general scale ; so that we continue to have the same confidence as before, in the scale of the thermometer, as a scale of the differences of heat. We have learned, that when ice melts, or when water boils, the cause of the phenomenon is the same with that of expansion ; because we observe, that what produces a double or triple expansion, also produces a double or triple liquefaction, or vaporisation ; and that congelation and condensation accompany contraction, which is always produced by the abstraction of what was the cause of expansion. But we see reason to think, that the manner in which the general cause of all these phenomena acts, in producing its effects, is somehow different. We find, that while the action of heat expands a body, it does not melt it ; and when it melts it, it does not expand it. If we conceive heat to be a substance *sui generis*, susceptible of union or combination with bodies, similar or analogous to those combinations which the chemist observes between numberless varieties of substances, we must conclude that the union which produces expansion is somehow different from that which produces liquefaction, &c.

When we perceive that what we call heat disappears in the liquefaction of ice, and reappears in the congelation of water, and a number of analogous phenomena, we can hardly avoid thinking it a substance, which may be united with the particles of water, in the same manner as the particles of Glauber's salt are united with them in solution, and may be separated as these are. But, since heat has never been observed by us in a separate state, all our notions of this union must be hypothetical. Moreover, this hypothesis must be combined with some other hypothesis, which we may have adopted concerning the unions of those other substances ; for it must be acknowledged, that our notions of those more palpable and familiar combinations are all hypothetical. We think that we can con-

ceive how a particle of common salt can draw around it, and attach to itself, particles of water, and how they may adhere, and compose a saline crystal.....We transfer this notion to the relation between bodies and heat, and (without much reflection, or distinct conception) we suppose, that, in like manner, a particle of a body attaches itself to a number of particles of heat. Heat is, therefore, supposed to be somehow contained or lodged in the pores of bodies, and we endeavor to account for the changes of sensible appearance, such as increase of bulk, or fusion, or vaporisation, &c. by shewing some resemblance between those appearances, and those which occur in chemical unions or mixtures.

Many have been the speculations and views of ingenious men about this union of bodies with heat. But, as they are all hypothetical, and as the hypothesis is of the most complicated nature, being in fact a hypothetical application of another hypothesis, I cannot hope for much useful information by attending to it. A nice adaption of conditions will make almost any hypothesis agree with the phenomena. This will please the imagination, but does not advance our knowledge. I therefore avoid such speculations, as taking up time which may be better employed in learning more of the general laws of chemical operations. I content myself with saying, that in liquefaction and vaporisation, water absorbs a great quantity of heat, because this expression immediately raises the notion of a sudden, and somewhat copious, accumulation of heat. And I say, that this great quantity of accumulated heat is *latent* in the water, or in the vapour,—merely because the thermometer, our usual test, does not give us any indication of its presence; of which presence we are not allowed to doubt, when we see the same quantity emerge again, in its usual thermometrical activity, when the water freezes, or the vapour collapses. Without saying that I have any clear conception of the union between bodies and heat, I am well entitled, by the phenomena, to say, that this vaporous combination differs in some peculiar manner from that which merely produces expansion. And, because I find that vapour cannot exist without this latent heat, any more

than aquafortis can exist without nitrous acid, I consider latent heat as constituting a part of aqueous vapour.

Some chemists think that the phenomenon of the production of elastic vapour is brought about in a different way. Almost immmediately after my discovery of this remarkable absorption and concealment of heat, in the phenomena of fusion and ebullition, Dr. Irvin, whose speculations were always ingenious, conceived that there was no occasion for the distinction between sensible and latent heat, in the peculiar circumstances of these phenomena. He imagined, that in every case there is a portion of heat which does not affect the thermometer; and that the reason why there is a seeming disappearance in liquefaction and ebullition is, that when the element changes its form from that of water to that of vapour, its capacity for heat is suddenly increased; and, consequently, it requires much more heat to be thrown into it, in order to make it appear equally hot by the thermometer. If it were in our power, by a wish, to convert an ounce of water, of the temperature 50°, for example, into vapour, without adding heat to it; this vapour, if examined immediately by a thermometer, would appear excessively cold, because the heat already in this ounce of water is not sufficient to heat an ounce of vapour to this temperature;....its heat is that of an ounce of vapour of an incomparably lower temperature; and it would therefore affect the thermometer as such, that is, would rob it of its heat, or cause the liquor to contract. The vapour has a much greater capacity for heat than the water from which it was produced. Heat will, therefore, flow into it from the surrounding bodies, till they are reduced to an equilibrium of temperature. And, on the contrary, when the capacity of the vapour is diminished, for example, by violent compression, and it is changed into water, this heat is redundant; the substance has not capacity for retaining it; it must be thrown out, as water is squeezed out of a sponge.

This is an ingenious thought, and gives a very simple and intelligible view of some of the appearances: but it does not assign a cause for the formation of vapour. The absorption or disappearance of the heat is here considered as

the consequence of the formation. The vaporous form must precede the absorption, if a greater capacity for reception of heat be the cause of this absorption.

Although this attempt to explanation, by an increase of capacity seems to have given very general satisfaction, I acknowledge that it does not appear to me to be any thing more than a different expression for what I have called absorption. There was surely something in the water, which made it unsusceptible of more heat, without rising in its temperature, and that something is now removed, and the water is now capable of receiving more heat without sharing it with a thermometer. But, if by capacity we are to understand more space in which heat may be lodged, I apprehend that we are speaking without sufficient authority. The same explanation is given of liquefaction, although it be well known that many substances occupy less room in their fluid than in their solid state*.

Next to Dr. Irvin, the ingenious Dr. William Cleghorn applied his peculiar theory to the same subject, and shewed that it is remarkably adapted to it. This theory is pretty much the same with the opinion generally entertained by the modern dynamical philosophers, as they are called, on the continent, who consider heat as consisting of particles mutually repellent, and strongly attracted by the particles of all bodies. The different kinds of matter will attach to them different proportions of this substance, in consequence of the different strength with which a particle of each substance attracts a particle of heat. The chief limit to the quantity attracted is this repellent power between the particles, by which it becomes a sort of elastic fluid. The more that its particles are constipated by the attraction of the particle of other matter which accumulates them round it, the more active must their mutual repulsion be. And thus, when the attractive force of any substance for heat is given, the quantity that can be constipated by it in a given space is limited. Each body, while it receives

* I would ask the partisans of this doctrine of capacity how it will explain the bursting of vessels by steam?

heat, is supposed to attract and condense more or less of these mutually repelling particles according to its strength, and when this is in equilibrio with the whole of their mutual repulsion, no more will be collected from the surrounding bodies, which have also their respective attraction for them. But when this equilibrium is attained, in the case of water, for example, if we suppose it suddenly converted into vapour, the particles of heat in it, having now much more room, are much less constipated round any one particle of the vapour; therefore, the repulsion of one of the particles, for the others surrounding that particle of vapour, must be greatly diminished, by their removal to a greater distance. But the watery particle, having still the same, or nearly the same attraction for heat as before, can now accumulate many more around itself, before their mutual repellency balances its attraction. They will, therefore, be drawn from the neighbouring bodies, and while this goes on, the vapour must appear colder than them.   Dr. Cleghorn adduces, in proof of this theory, the well known fact, that a thermometer, in the receiver of an air-pump, descends when the air is rarefied, and rises when it is condensed.

There is no doubt but that this supposition agrees pretty well with this single phenomenon.   We can form a pretty distinct notion of it; but, like the former, it gives no explanation of it. It does not shew us how the gathering a certain quantity of heat round the particles of water should be followed by an immediate and great separation of those particles from each other, each carrying with it its due proportion of calorific matter, so as to leave room for the reception of a great deal more : far less will it shew us how the abstraction of the smallest portion of this additional heat by a colder body, instantly brings all the particles of the vapour together, again expelling the heat.   An active cause for the change of form is wanted, before we can speak either of enlarged capacity, or less repulsion.

Many other strong objections may be made to this explanation.   Some of these will occur when we consider the nature of chemical union ; and it is needless to dwell more on this speculation, because it gives very little add-

tion to our chemical knowledge. I content myself with say-ing, that it is much more reasonable to conclude, that the absorption and entrance of this great quantity of heat into the composition of vapour is the cause of its formation, than that it is its consequence; because we have the greatest reason to be satisfied that heat, in one shape or other, is the princi-ple or only cause of the formation of vapour, as it is of fluid-ity*. *(See Note 5. at the end of the volume.)*

I have already observed, that the heats which produce va-pour from bodies under the pressure of the atmosphere, are, with regard to the greatest number of them, higher than those which produce simple fluidity. This is, however, not the case with respect to all kinds of bodies. There are some solid substances, which, when we heat them, evaporate en-tirely, or are totally changed into vapour, without previously melting, or shewing any tendency to become simply fluid; and the vapour into which they are changed, when cooled again condenses into solid matter, not into a fluid. Such are camphor, the volatile alkali, sal ammoniac, arsenic, corrosive sublimate of mercury, and some others. These bodies, how-ever, are capable of being brought into a melted state, by in-creasing the pressure on their surface above that which they commonly sustain from the weight of the atmosphere, which must be done by confining them and their steams in close vessels. The consequence of this is, that they are forced to bear more heat, and several of them can thus be made so hot as to become perfectly fluid. This shews, therefore, that the principal difference between them and other bodies is, that whereas other bodies are more easily melted than changed into vapour by heat, under the ordinary pressure of the air, these, on the contrary, are sooner changed into vapour than melted, and some of them so much sooner, or with so much less heat, that we dare not venture to confine them so long as is necessary to learn what degree of heat is requisite for their fusion. They are so volatile that they would burst

* See Lavoisier's Mem. 1777, in which he explains his notion of the man-ner in which all elastic fluids are formed of some solid matter, and the mat-ter of heat.

any vessels in which we could attempt to make the experiment.

These differences of bodies from one another will enable you to understand the nature of several chemical operations, which depend upon the production of vapour; such as *evaporation*, *distillation*, and *sublimation*. These are operations occasionally performed with mixtures of bodies, or compound substances, the different ingredients of which we desire to separate from one another; and they can only be performed with those mixtures or compounds which contain ingredients differing in volatility, and which are not united together by a strong attraction. When this is the case, all that is necessary is to expose the whole to heat, very gradually increased, so that the more volatile may be gradually converted into vapour, and wholly separated, before the more fixed. It generally happens that the separation effected in this way is not complete. The action of heat in the conversion of a body into vapour, seems not to destroy altogether the former combination. A very scanty supply of heat promotes the separation, seeming to allow the difference of volatility to produce its effect with greater certainty. This however seems a difficulty. An operation of this kind is called evaporation, when performed with a view to obtain the more fixed parts, and when we are careless about the more volatile, and therefore expose the matter to heat in open vessels, and allow the volatile matter to fly away in the air, as in the manufacture of common salt: but in distillation and sublimation, we preserve the volatile parts, which is done by performing the operation in vessels and instruments, by which the vapours are not allowed to escape, but are directed into a part of the apparatus in which they are cooled and condensed again. In this, distillation and sublimation agree, and the distinction between them depends on the state and appearance of the condensed matter produced from the vapour. In distillation, this condensed matter is a fluid; in sublimation, it is a solid substance. In one of these operations, therefore, we work upon subjects that are more fusible than volatile, or more easily melted than volatilized; in the other we work upon those that are more volatile than fusible.

There are several other terms which might be taken notice of here, but it is not to be supposed that you would remember them, though I should enumerate and explain them at present. I shall have occasion to notice them hereafter.

Beside thus explaining these chemical operations, the doctrine of vapour will enable you to understand many particular effects which heat produces on certain bodies of a compound kind; such as entirely changing their appearance and properties,....converting them, as it were, into new substances. This happens, because such compound bodies consist of ingredients which differ in volatility, and cohere weakly. Heat dissipates some in the form of vapour; and both the volatile and more fixed ingredients combine in a new manner. Hence wood contracts, instead of expanding; and instead of melting afterwards, is converted into water, oil, salt, charcoal, earth. And clay contracts always the the more it is heated, and remains ever after in the contracted state.

Nothing general can be delivered on this head, because these are all instances of peculiar properties. We can only see clearly, that substances, which are brought into particular states of combination by the operation of nature or art, may continue in that state unchanged, while their temperature remains the same. But when we raise this to a degree which volatilizes several of their ingredients, and when these are such as will unite, either in this high temperature, or in a lower, when each has collapsed, it must happen that we shall produce compounds which did not exist in the body from which they have been raised. And it may happen, that the ingredients that are left behind, are such as will unite, but had taken another combination, by the intervention of the ingredients now volatilized. We shall thus obtain other compounds which did not exist in the body.

These results appeared very wonderful, while the science of chemistry was in its infancy, and led into many mistakes as to the compositions of bodies. The detection of those mistakes, constitutes the chief part of the advances which

have been made in the science to this day, and will occupy the greatest part of the following course of lectures.

It remains now to be considered, whether this effect of heat may be supposed universal with regard to all bodies, or whether there are any exempted from this power of heat. Many philosophers are inclined to suppose that it would be found universal, were it in our power to employ heat of the utmost intensity; and we have, no doubt, very extensive experience of the possibility of converting into vapour a great many of the different kinds of matter, many of them such as might not at the first view seem much disposed to undergo this change; such as all the different kinds of salts, and the inflammable substances, and the metals. But still we find much difficulty with respect to the earthy bodies. The greater number of these shew no disposition to volatility, but on the contrary, resist the action of our most violent fires, without any signs of their being changed into vapour. There are, however, one or two of them that, in very violent heats, are more or less dissipated; and from these examples, some may perhaps imagine that the rest might likewise be volatilized, were it in our power to command and employ more violent heats; but this is only a supposition.

Some philosophers have taken such an enlarged view of this power of heat, that they have not only supposed the most fixed bodies might be volatilized, had we a sufficient heat in our power, but that every palpable, elastic, fluid in nature, is produced and preserved in that form by the action of heat. Mr. Amontons, an ingenious member of the late Royal academy of sciences at Paris, was the first who proposed this idea with respect to the atmosphere. He supposed that it might be deprived of the whole of its elasticity, and condensed, and even frozen into a solid matter, were it in our power to apply to it a sufficient cold; that it is a substance which differs from others, by being incomparably more volatile; and which is therefore converted into vapour, and preserved in that form, by a weaker heat than any that ever happens or can obtain in this globe, and which therefore cannot appear under any other form than the one it now wears,

so long as the constitution of the world remains the same as at present.

This opinion, though it may appear, at the first view of it, to be an extravagant flight of the imagination, is, however, supported both by analogy, and, in some measure, by direct experience. We know that water is easily convertible by heat into a vapour, which, so long as it is kept sufficiently hot, has many of the qualities of air. It is very light and rare, even more so than air, and it is equally transparent. It is also very compressible, and reacts with an high degree of elasticity against the force compressing it, in the same manner as air; and its elasticity is greatly increased by additions of heat, as well as diminished by the abstraction of it.

As heat can thus produce from water an elastic vapour, which resembles air in so many of its qualities, it is not improbable that air itself is produced in the same manner from some other matter. The presumption is very strong; and, when we consider the effects of heat on air more attentively, we find reason to be the more presuaded that its rarity and elasticity depend upon heat; for the more its heat is increased, it becomes always the more elastic, and, if it has room to expand, the more rare: and although we are not able to cool it so much as by that way alone to reduce it to a dense unelastic body, we find, however, that the more it is cooled, its elasticity is the more diminished; and it is the more condensed, without any bounds to this condensation and diminution of elasticity, except our inability to make it colder.

In the modern doctrines of chemistry, therefore it is now considered as an indubitable truth, that the air has its rare and elastic form from the heat it contains; and it is assumed as an established principle, that all other permanently elastic fluids derive their elastic vaporous form from the same cause; that each of them is a compound of a peculiar matter, and of the matter of heat, united in the state of latent heat, (or caloric, as it is named by the French chemists,) with the other matter, so as to constitute with it an elastic fluid or vapour. I observe, that Fourcroy, in his *Elemens de Chymie*, says expressly, that by CALORIQUE, his countrymen mean the same

thing that I named latent heat, and that they reserved the common word CHALEUR to express moveable or sensible heat.   I am not altogether certain of Fourcroy's being sufficiently warranted in this use of the words, nor do I think it the most proper.   Were I to write French, I would use the term *chaleur latente* for those combinations from which heat is abstracted by the touch of a colder body, and I would restrict the term *calorique* to the combinations in which it is indeed equally concealed, but from which it cannot be extracted, but by presenting a *proper* third body, at the same, or even at a higher temperature, namely, to the gaseous combinations, whence it is generally disengaged, accompanied with light, exhibiting the phenomenon called combustion.   I am the more disposed to make this distinction, because I think that this is a general mode of combination, containing several different steps, analogous to those observed in the combinations of latent heat.   But of this we shall occasionally hear more afterwards.

OF EVAPORATION.

Thus, we have come near the end of this subject; but to make it complete, it is necessary to observe, that, besides the elastic steam produced from volatile bodies, when a sufficient quantity of heat is thrown into them, there is a different kind of vapour produced from many of them, in much lower degrees of heat than what is necessary to the production of the former.   But this second sort of vapour is always produced slowly and imperceptibly without the smallest commotion or agitation, and only occasions a very slow and gradual diminution of the mass, until it be entirely changed, and disappear. Thus, camphor, or volatile salts, even though wrapped in paper, or other such containing matters, are continually emitting vapours, which are perceptible to the smell, though invisible to the eye, and are thus gradually wasted, until they are entirely lost.

We have a familiar instance of this sort of vaporisation, as well as of the other, in water.   If water be exposed in open vessels, it is sure to exhale away imperceptibly, in this man-

ner, until it totally disappears, whatever be the temperature
of the air to which it is exposed; faster, indeed, as the
weather is warmer; but it has been observed, that even in
frost, and when the water itself is in the form of ice, it always
suffers some loss this way.

This, to distinguish it from the former, may be called NA-
TURAL EVAPORATION. It happens through a considerable
latitude of heat, but becomes more remarkable, as the heat is
increased. The vapour thus produced, differs, by an obvious
distinction, from the one I have hitherto described, and which
may be called the *elastic vaporisation*, which is produced only
by increasing the heat to the boiling degree. The vapour,
produced by the boiling heat, and which may be called steam,
has great elasticity and force, and requires violence to confine
it; whereas that produced in natural evaporation, unless it
be made as hot as the other, has no elasticity that is percepti-
ble in ordinary circumstances, but may be detained in the
weakest vessels, with ease, and does not exert the smallest
sensible force upon the vessel which confines it. Every body
knows that water can be preserved for any length of time in
close vessels of glass, however thin. Provided they be such
as that the water cannot penetrate or dissolve them, it never
will be diminished in quantity, nor will it produce any vapour
that will shew the smallest disposition to burst them. As this
sort of evaporation of water exposed to the air goes on always
in a quiet and imperceptible manner, without any commotion
or agitation of the fluid, it is plain that it must proceed en-
tirely from the surface : and accordingly, it appears by expe-
riments that it is always more copious, as the surface is grea-
ter.

Another observation, with regard to this kind of evapora-
tion, must not be omitted ; that if this sort of vapour be allow-
ed to stagnate over the body, or be confined upon its surface,
it proves an obstacle to the formation of any more, or to the
progress of evaporation. In order to have a continued pro-
duction of this vapour, it is necessary that it be carried off, or
removed from the surface as fast as it is formed.

Hence wind greatly promotes natural evaporation. I like-
wise observed before, that it is greatly promoted by heat.

The production of this vapour does, indeed, so much depend upon heat, that when, in consequence of more heat than ordinary applied to bodies, a great quantity has been produced, it is condensed again, like the other, when this heat is diminished. Thus, when a bottle or glass of cold water is brought into the room, where many persons have been sitting for some time, its outer surface is immediately dimmed by the vapour contained in the air of the room, now condensed upon it. Hence, too, arise the moisture and hoarfrosts, on the inside of our windows, in very cold weather; as also on our walls, when a hard frost is followed by a sudden thaw, brought on by a wind coming from the south, and filled with vapour. It is a vulgar error to ascribe this moisture on our stone walls to the weather penetrating through them. It is the vapour contained within the house, condensed by the cold walls. We observe none when the walls are covered with laths and plaster, and think that this prevents the penetration. But the true reason is, that this thin, detached covering, soon acquires the warmth of the house, and condenses no more vapour. The glass of our windows condenses it copiously, because very cold, yet it is impenetrable by the weather.

From the same cause arise the mists, which hover over low marshy grounds in the cold mornings and evenings, especially after a warm day, in which the evaporation has been very copious. In this case, the smallest diminution of heat produces a condensation, and the vapour becomes visible, and is precipitated. The mist seems to rise, and we see a stratum of clear air below the stratum of dew, because the precipitation begins in what is next the ground, and proceeds gradually upwards, as the strata cool in succession. For the same reason, we frequently see a smoke apparently rising from holes made in the ice which covers deep lakes and rivers. (See Note 6. at the end of the volume.)

This sort of vaporisation, and particularly the natural evaporation of water, has been much attended to, as being closely connected with some of the more important and extensive operations of nature.

By this sort of vaporisation, water arises into the atmosphere, from the surface of the sea, and other large collections, in the form of transparent vapour, which, at a certain height, is condensed by the cold of the superior regions, into mist or clouds. These clouds are suspended at different heights, probably in consequence of the particular temperature and rarefaction of that portion of air which is contained in them, and they are liable to become electrified, so as sometimes to occasion lightning and thunder. Often also, in consequence of a more quick and perfect condensation of the vapour, or by means of an electrical attraction of the little globules of water for one another, they form drops of water, which descend in rain, which is indispensably requisite for the nourishment of vegetables and animals, and gives origin to springs, rivers, and lakes.

As this circulation of water, by which we all live, depends entirely on natural vaporisation, it has greatly attracted the attention of philosophers, and has occasioned many attempts to discover and explain in what manner it happens.

1. The first remarkable attempt was made by Dr. Derham, in his *Physico-Theology*, B. I. ch. 2. and B. II. ch. 5. He says that fire, being the lightest of all natural things, rises up through the water, and carries off with it a quantity of water. The fire, bursting out of the water, is enveloped with a thin film, in the form of a small bladder, or vesicula, which rises in the air by its extreme lightness, till it attains a height corresponding to its specific gravity. Myriads of such vesiculæ form clouds, which float about in the atmosphere, till they are condensed by cold, or coalesce by being driven against each other by the winds. Then they descend in dews, fogs, rains, snow, or hail.

This was a very careless attempt to explain so curious and important a phenomenon. Vesicles of such a constitution could not remain a minute in the air, but must collapse as soon as they are formed. Nay, we cannot conceive their formation. Fire has never been met with in the form of a palpable expansive fluid, like air, fit for distending these vesicles. They must instantly collapse, acquiring the temperature of

the air; and now, being much heavier, must immediately fall down again.

2. The theory of Desaguiliers is founded on calculations of the space which he supposed water to occupy, when changed into vapour, with different degrees of heat. He first endeavors to calculate how much, or to how many times its own bulk, water is expanded, when changed into steam by the boiling heat, or elastic vaporisation, under the pressure of the atmosphere; and, from some premises, he concludes that, when changed into steam, under that pressure, it is expanded to 14000 times the bulk of the water. In the next place, he supposes that the different degrees of heat, between frost and the ordinary boiling point of water, must produce from water vapours less rarefied, but still very rare, and approaching in rarity to the steam of boiling water, in proportion as the heats by which they are produced approach to that heat, and are distant from frost, or the cold of melting snow. And, from these principles, he concludes, that the heat of the weather in summer, spring, and autumn, and even in the greater part of winter, is still sufficient to form vapours from water, which shall be lighter than air; and therefore disposed to ascend, or to be driven upwards in the atmosphere.

But his very first proposition is totally false, and the falsity of it overthrows the whole of his reasoning. Water, when changed into steam, is not expanded into 14000 times its ordinary bulk. I examined, with great attention, that part of his book in which he had stated this proposition, to see if I could learn on what it was founded. I could not obtain the least satisfaction: and I can assure you, that, by a simple and decisive experiment, which I lately described to you, the space which water occupies, when changed into steam, under the ordinary pressure of the atmosphere, is only about 1664 times its bulk, when in the form of water. More recent experiments by Mr. Watt, make 1720, or rather more; but he is not perfectly precise in his opinion.

3. A much more ingenious explanation was proposed by the celebrated astronomer and philosopher Dr. Halley. In some of his writings, indeed, he gives Derham's explanation of the

rise of vapours by vesicles, distended, not by fire, but by heated air. (See Phil. Trans. No. 192.) This is as untenable as the other. But Dr. Halley, afterwards, in a dissertation on the origin of springs and rivers. (See Phil. Trans. abridged, vol. 2, No. 127,) expresses himself as follows :

" I take it that the air itself will imbibe a certain quantity of " aqueous vapours, and retain them, like salts dissolved in " water. The sun warming the air, and raising a more plentiful " vapour from the water in the day-time, the air will sustain " a greater portion of vapour, just as warm water will hold " more salts dissolved, which, upon the absence of the sun in " the nights, will all be again discharged in dews, analogous to " the precipitation of salts on the cooling of the liquors."

It is surprising that an explanation, suggested by so eminent a philosopher, and supported by so extensive analogy, did not meet with more general acceptance. Although this essay of Dr. Halley's, and another much connected with it, are frequently quoted with the greatest respect, especially on the Continent, this passage is never mentioned. And in 1743, when the Academy of Sciences of Paris published this as a prize question, the many performances of the competitors, and even those of Kratzenstein and Hamberger, which shared the prize, take no notice of it. But the explanations given by these authors are very inadequate indeed ; yet the theory of Kratzenstein, who adopts the vesicular form of the matter which composes cloud, fog, or dew, is accompanied by many very curious observations and ingenious reflections on these vesicles. He produces the vesicles, by forcing a quantity of air into a clear glass globe from the lungs ; the globe has a leather valve, which keeps in the air ; when this is pierced with a pin, the air comes out slowly, and the vesicles appear in the glass in strata, exhibiting beautiful colours. In 1750, however, the same Hamberger, in his *Elementa Physices*, published at Halle, adopts Dr. Halley's solution-system completely, but without seeming to have taken it from him, so unskilfully does he manage it.

In the following year, Mr. Le Roy, physician at Montpellier, (I think) and correspondent of the Academy of

Sciences, gives this system in full detail, supported by a nume-
rous train of resemblances to the solution of substances in
their solvents, and applied to the chief phenomena with great
distinctness and ingenuity. The celebrated Dr. Franklin, the
late Lord Kaims, and Dr. Hamilton of Trinity college Dublin,
have also published dissertations to the same purpose, sup-
ported by the same arguments, and applied nearly in the same
manner.

The circumstances adduced by these authors, in which the
rise of vapour, and the principal phenomena of the atmosphere,
resemble chemical solutions, are indeed very many and very
specious.

There is an actual solution, for the dampest air is perfectly
transparent : bodies dry very fast in warm air, but slowly in
cold, and in the cool of evening, the dews are more copious
after a warm day. Every temperature of the air appears to
have an appropriate power of taking up vapour, and part is
precipitated as it cools. This perfectly resembles the satura-
tion which obtains in all solutions. Extent of surface greatly
promotes evaporation. More water will evaporate from a
moist cloth in an hour, than from a bowl of water in a day.
Wind also greatly promotes evaporation, just as a renewal of
water or agitation promotes solution. Stagnation, on the con-
trary, puts a stop to both.

To these arguments may be added, that evaporation pro-
duces cold, in proportion to its extent and rapidity, just as it
is produced in the solution of salt.

This theory of the ascent of vapours, when thus proposed,
soon obtained a general concurrence, and was freely employed
by the meteorologists. But an experiment related by Walle-
rius, in which he observed a copious evaporation in the ex-
hausted receiver of an air-pump, raised some doubts about the
validity of this explanation. The experiment has been repeated
with great care by many philosophers, and it is now well
known, that evaporation goes on much faster *in vacuo* than in
the open air, and is more and more retarded, in similar cir-
cumstances of temperature, &c. in proportion as the evaporat-
ing body is surrounded by denser air. It is found that heat

expands water into elastic vapour in all temperatures. In an experiment by Mr. Watt, water of the temperature 57° produced a vapour which supported half an inch of mercury. This experiment was a very curious one. A single drop of water was let into a very fine barometer ; as soon as it got above the mercury in the tube, the mercury sunk half an inch. Mercury was now poured into the cistern, and it rose also in the tube; but the mercury in the tube still wanted half an inch of its first elevation above that in the cistern. This depression continued while mercury was still poured into the cistern, till at last the vapour of the water was all condensed again. This is analogous to an experiment which I mentioned some time ago, in which boiling hot steam was condensed by pressure.

Now, in this experiment of Mr. Watt's, there was no air to dissolve the water. Mr. Nairne produced, in like manner, vapour of the same strength in the air-pump receiver, from a bit of moist paper. Mr. Saussure set a barometer into a tall glass, the air of which was rendered perfectly dry by means of quick-lime, which absorbed all moisture. He then threw into it a small piece of moist linen, and instantly shut the apparatus. In a short time, the vapour produced from the cloth raised the barometer half an inch. Comparing these three experiments, we see that the presence of the air, in the last experiment, had not contributed to the production of vapour.

It is therefore considered as very doubtful, at least, whether the active dissolving power of the air is the cause of the rise of vapour in the atmosphere ; and it seems to be the most prevailing opinion that heat is the sole agent. Those who continue to maintain Le Roy's system, (Mr. de la Place and Mr. Monge) say that there is less cold produced in spontaneous evaporation in the air, than in the artificial evaporation *in vacuo :* but another experiment by Mr. Watt shews the contrary. Two vessels, perfectly similar, were exposed, filled with water of the same temperature ; the evaporation from one of them was prevented by a covering of oil; the heats lost by both were compared, and the grains of water evaporated were measured ; the difference of the heats lost greatly

exceeded the heat lost by evaporating the same quantity of water in the air with a boiling heat. Thus, the solution-sys-tem seems to lose ground. Mr. Saussure thinks that heat alone expands the water into vapour, and that the incumbent air dissolves this vapour. Mr. De Luc has another theory, but it is too complicated to be considered here. See his *Meteoro-logie.* *(See Note 7, at the end of the volume.)*

I mentioned the cold produced by spontaneous evaporation. Dr. Cullen's experiments, which I adduced on another occa-sion, are the most remarkable examples. Mr. Mairan and Mr. Franklin give many examples of the cold produced by the evaporation of water and other fluids from the skin. It is from this cause that a wet cloth exposed to the air, often freezes stiff. Although the thermometer stands some degrees above 32°, water evaporates, and this cools the remainder so as to freeze it.

When these phenomena were first taken notice of by our philosophers, they were thought very wonderful; but they have been long familiarly known in India and in China. In those sultry regions, water is kept cool by putting it into very porous earthern jars. These are ever covered with moisture, in a state of copious evaporation. The traveller carries water in an earthern mug of the same kind, or covers his can with woollen cloth ; he has this slung on his shoulders, and dips it into every pool or puddle. The land-winds being excessively dry, cause the water to evaporate very fast, and a great deal of heat is thus absorbed. The wealthy have their dining halls open on all sides, the roof being supported on pillars, and the intervals are hung with curtains. Servants without doors scatter water continually on these curtains, and its evaporation absorbs a vast deal of heat, or makes the hall cool and refreshing.

Nay, such is the effect of evaporation in absorbing heat, that the rich inhabitants of those countries can lay up stores of artificial ice, in a climate where the thermometer is never under 40°, and frequently rises to 110°. I shall explain the curious process for this purpose, as I have it from a gentle-man who resided nine years at Benares, one of the ancient

seats of arts and sciences. He saw the process performed
several times, and his letter on the subject is to be seen in the
philosophical transactions for 1792 or 1793.

Benares is between the 25th and 26th degree of north lati-
tude, a great way up the Ganges and has an immense continent
extending from it in every direction. The process is per-
formed in the middle of their winter only, and in clear dry
weather, when the thermometer falls to within 8 degrees of the
freezing point; and although no ice is ever formed on the
pools and other small collections of water, they succeed in pro-
curing it by this process.

Shallow pits or beds are made four or five feet wide, and
about four inches deep, separated from one another by narrow
borders, and so numerous as to cover an extent of about four
acres. These pits are filled with dry straw in the proper
season, and on the straw are placed rows, close together, of
shallow earthern pans, into which a few inches depth of water
are put every evening of the winter season, when the weather
is clear, and the nights very cool. In the morning they find
more or less ice in the pans, which is taken out before sun-rise,
or about five o'clock; and that it may seperate the more easily
from the earthern vessels, the inside of these is rubbed with a
little butter before the water is put into them. The number
of earthern vessels or pans in one place is about 100,000; and
about 300 men, women, and children, are employed in the
morning in gathering the ice, that it may be done quickly,
before the heat of the day begins. They wrap it in thick
coarse woollen stuff, and carry it to the ice-house. The clear
and cold weather, which is necessary to the production of the
ice, by this process, is undoubtedly attended with a northerly
wind. And in such a country, the northerly winds, or
breezes, are excessively dry, in consequence of their coming
over a great extent of dry northern continent. They have,
therefore, great power to promote evaporation. The shallow
earthern vessels, exposed in this maner, and the water in them,
must become colder, by the quicker than ordinary evapora-
tion of a part of the water, than any other matter around
them. Care also is taken, by the contrivance of the process,

to prevent the coldness of the vessel and water from being too much diminished by communication with the warm soil that is below. The thick bed of straw, on which the vessels are laid, is evidently intended to serve this purpose.

In another part of India, farther south, or nearer to Calcutta, they find more difficulty in preparing ice by a process of this kind. But experience has taught them, that boiling the water, before it is exposed, promotes their success. This fact was communicated to me in the year 1774, or 1775, by the late Sir John Pringle, president of the Royal Society, who desired to have my opinion on it. Before I gave any opinion, I thought it was proper to enquire into the certainty of the fact, and I accordingly made some experiments relating to it. But I cannot say that they terminated in any very confident opinion. I found very little difference, if any, in the quantity of ice produced from boiled and unboiled water. I think that, in most of my trials, the boiled water first began to freeze, and, when the heat of the air was but very little below 32°, suppose not lower than 29°, the difference was certain, and more considerable. I found always, that when the one had frozen a little, and the other not, the smallest touch or disturbance, with a tooth-pick, made it freeze instantly. The difference, therefore, which I think may be stated between them, is that boiled water begins to freeze as soon as the air has cooled it to 32°, but that unboiled water will continue fluid a little longer, if not disturbed. I recollect that the experiment in which this curious fact was first observed, was made by Fahrenheit, with water which had been purged of air by boiling. He had sealed the vessel containing it. Wishing to apply a thermometer, he broke off the end of the neck, and the water froze as soon as the air touched it. Perhaps this was effected by the water's beginning immediately to absorb air again. Perhaps air, as well as salt, retards the congelation of water.

On the whole, this operation is an example of a curious art, which in its present state of perfection and simplicity, seems the product of great knowledge and ingenuity, requiring uncommon talents in any individual to invent and direct it. But it probably even began in ancient times, in a cooler cli-

mate, and extended slowly southward, in consequence of repeated improvements, derived from experience, the source from which all our useful knowledge has proceeded.

I am persuaded that the heat absorbed in spontaneous evaporation greatly contributes to enable animals to bear the heat of the tropical climates, where the thermometer frequently continues to shew the temperature of the human body. Such heats, indeed, are barely supportable and enervate the animal, making it lazy and indolent, indulging in the most relaxed postures, and avoiding every exertion of body or mind. The inhabitants are induced to drink large draughts of diluting liquors, which transude through their pores most copiously, carrying off with them a vast deal of this troublesome and exhausting heat. Still however, these heats are most oppressive to the poor, who must work, and often produce great mortality, when they continue long unabated. There is in the body itself a continual laboratory, or manufacture of heat, and, were the surrounding air of such a temperature as not to carry it off, it would soon accumulate so as to destroy life. The excessive perspiration, supplied by diluting draughts, performs the same office as the cold air without the tropics, in guarding us from this fatal accumulation. It is not unlikely that that constitution of the vessels of the lungs, and the pores of the skin, which unfits them from bringing forward the lymph, in sufficient quantity for carrying off, by evaporation, the heat generated by the vital functions, is the immediate cause of the heat, in ordinary fevers.

It is really wonderful what heats may be carried off in this way. Dr. Fordyce staid for a considerable time, and without great inconvenience, in a room heated to 260° of Fahrenheight's scale. The lock of the door, his watch and keys lying on the table, could not be touched without burning his hand. An egg became hard; his pulse was at 139°; yet a thermometer held in his mouth was only 2° or 3° hotter than ordinary. He perspired most profusely. (See many curious and instructive observations and reflections on these experiments in Phil. Trans. vol. 64. and 65.)

There is only one phenomenon remaining that may be thought worthy of notice, and it was discovered by the worthy and ingenious Professor of Astronomy at Glasgow, Mr. Wilson, and described by him in a paper of the first volume of the transactions of the royal Society of Edinburgh: and he also communicated some of his observations on this subject to the Royal Society at London.

During a winter, when the cold was uncommonly severe and intense in this country, and when the thermometer fell some nights to the beginning of Fahrenheit's scale, and, even in the day time, stood for many hours at 10 or 12 degrees only above it, the Professor, who passed some part of the night in the Observatory, took with him several thermometers, and placed them in different situations. Some were laid on the surface of the snow, others hung up in the air, and otherwise; and he very soon perceived, that those which were laid on the surface of the snow, and he very soon perceived that those which were laid on the surface of the snow fell considerably lower than the rest; sometimes they were 14 degrees lower. He then sunk some of them below the surface of the snow, which was uncommonly deep, and found, as he expected, that they rose and stood higher, than when they were hung up in the air; they rose the higher the more deeply they were sunk in the snow, and the nearer they were placed to the surface of the ground. This was easily understood, but the surprising and unaccountable fact was, that they should indicate a greater degree of cold when laid on the upper surface of the snow, than when hung up in the air above it. This fact excited his curiosity, and he made a great number of judicious experiments to discover the circumstances with which it was connected. He learned by these experiments, that it was connected with a disposition of hoar-frost, and that it was only when the thermometer was placed on a surface of snow or other matter, on which a disposition or condensation of hoar-frost was going on, that it sunk so low. This he ascertained by a number of decisive experiments: but he observed that it happened only when the cold of the air was at the same time

violent, so as to make the thermometer fall 20 degrees, or
more, below frost.   Still, however, it was a most unexpected
and surprising phenomenon.   From the knowledge we have
of latent heat, we might rather expect to see an *increase* of
sensible heat, on this occasion, than a *diminution* of it.   *(See
Note* 8, *at the end of the Volume.)*

SECT. IV.....OF IGNITION.

WE are now to attend to the fourth general effect of heat
commonly called IGNITION, or INCANDESCENCE.

By this term, is meant that glow, or emission of light from
bodies, which appear when their heat is increased to an high
degree of intensity, about the 650° of Fahrenheit's scale ; and
which, when it first begins, is weak and obscure, and of a red
colour, but increases in brightness as the heat becomes
stronger until, in the most violent intensity of heat which we
can produce, it becomes so bright and dazzling a whiteness,
that the eye cannot bear it.

A difficult question here occurs, concerning the connection
between heat and light : how happens it, that each is naturally
disposed to produce the other? We know by experience, that
whenever a body is heated intensely, it always becomes lumi-
nous, or emits light ; and, on the other hand, it appears as
plain, from the effect of the light of the sun, the most luminous
body in nature, and, especially, when this light is condensed
by convex glasses, or concave mirrors, that a strong light, di-
rected upon bodies, is sure to produce in them very quickly
the most violent heat.

The opinion of some of those who have ventured to form
conjectures on this subject, is, that light, as well as heat, is pro-
duced by that subtile, elastic, self-repellent matter, which has
been imagined to be the cause of the sensation heat ; That
when the matter, in its ordinary and most communicable state,
abounds in a body, the body appears hot, or is in a hot state ;
but that, when it abounds to a high degree, some particles of
this subtile matter are thrown off with inconceivable velocity,
corresponding to its high degree of elasticity, or the powerful

repulsion of its parts for one another; and that these projected atoms constitute light. *(See Note 9, at the end of the Volume.)*

Dr. Boerhaave considers the connection between heat and light at great length in his treatise on fire; but he perplexes himself, and gets into a confusion of ideas that is surprising. He takes much pains to find the reason,—why the light of the moon, though collected by mirrors, or burning glasses, should not sensibly affect the most delicate thermometer; while that of the sun, collected by the same means, produces very quickly the most vehement heat of which we have any experience, and has great affect on bodies even when it is not condensed. At last, he thinks he has found the reason of the difference, which is, that the rays of the sun are more parallel than those of the moon.

But this is the most unfounded notion that can be imagined. Neither the rays that come from the sun, nor those that come from the moon, are parallel to one another. Those, indeed, that come from any single point of either of these luminaries, are very nearly parallel when they reach us. On account of their coming from a point at such a very great distance, they are so nearly parallel, that they appear, by experiments, to be exactly so; and there is no sensible difference between the rays of the sun, and those of the moon, in this respect. Though the sun is more distant than the moon, the moon is at too great distance to admit of our perceiving the smallest sensible divergency in the rays that come from the same point of her surface. But if we consider the rays which come from different parts of the surface, either of the sun or of the moon, it is plain that they are far from being parallel. The sun's disk subtends an angle of half a degree; and his rays, if we consider the whole of them together, are not so parallel as those of the moon, whose diameter subtends an angle not quite so large. It is, therefore, surprising that Dr. Boerhaave should have taken up this idea, which, at any rate, does not in the least make the fact more intelligible. If the effect of the sun's rays, in heating the bodies upon which they fall, depend upon their parallelism, the light which comes from

Jupiter and Saturn, in some of their positions, should be more powerful than that of the sun.

The true cause of the great difference between the effects of the rays of the sun and those of the moon is the excessive feebleness of the moon's light.

The moon is plainly illuminated by the sun, and the light which comes from her to us, is a part of the sun's light reflected from her surface. But it is plain that, even supposing it to be all reflected, (which is very improbable) this reflection must happen in such a manner, that the light is dispersed equally in all directions; and therefore, a very small part of it only can come toward the earth; such a small part, that it is rather difficult to understand why she appears so bright. Accordingly, some philosophers think that the surface of the moon must be of the very brightest kind, as if it were covered with snow. But I do not think it necessary to form such a supposition, in order to account for her apparent brightness at night. I was surprised at the appearance which a baloon of oiled paper, filled with inflammable air, made in a serene blue sky, and bright sunshine, though the reflection of light from it, when near the earth, was not remarkable. It appeared like a star, or like Venus, when it was so high that its shape was not seen.

That the light reflected by the moon to our earth is, in reality, next to nothing, when compared with that of the sun, is plain from some obvious and easy calculations.

Bouguer's experiments were made by causing a beam of the sun's light to diverge by means of a concave glass. Thus the light was weakened as it receded from the glass, the strength of it being, at different distances from the focus of glass, inversely as the squares of these distances. Then he investigated, by experiment, the distance at which it was equal in strength to the light of a candle thrown upon paper, placed at proper distances from the candle. And he afterwards compared the light given by the same candle with that of the moon. In making this last comparison, it was not necessary to weaken the light of the moon, in order to make it equal to that of the candle. It was, on the contrary, necessary to weaken that of the candle. The final result of Mr. Bouguer's calcula-

tions is, that the sun's light is more than 300,000 times denser than that of the moon; which last is, therefore, quite undis-coverable by our most delicate thermometers.

Ignition may be considered as one of those effects of heat that is produced in the most similar manner, upon different bodies, of any kind whatever. It is also a very general effect of heat. We do not know of any bodies that are exempted from it, unless it be those whose volatility is so great, that they cannot be made to endure the degree of heat necessary for this effect. But even these, when in the form of vapour, may perhaps be heated red hot, though many of them must be so very greatly expanded before it acquire the necessary degree, that it would not be easy to perceive whether or not they are in the state of ignition. (*See note* 10, *at the end of the volume.*)

Upon the whole, we have reason to consider this as an effect of heat, to which all bodies, without exception, are liable, and all at the same, or nearly the same, degree of heat. But it is difficult to know exactly the place of this degree in the scale of heat. We depend on the acuteness of our sight to inform us of the beginning of ignition, and we are not sure that it per-ceives the very weakest light. The presumption is rather the contrary, as we know that other animals see objects in such weak light, as appears to us to be utter darkness. And even our eyes will, in one condition, be affected by a weak light, to which they are quite insensible in another.

Sir Isaac Newton, however, attempted to estimate the de-gree of heat, which is attended with the weakest perceptible light, by the method I formerly described, viz. exposing a lump of very hot iron to a stream of cold air, and observing exactly the time when it ceased to emit light; and having also observed the celerity and progression with which it cooled, or lost its heat, from the beginning of the experiment until it was reduced to the temperature of the air, he was enabled to esti-mate the heat which remained in it at the last moment of its ignition.

By a calculation of this kind, he reckons that iron, in the lowest state of ignition, or when it just ceases to shine per-ceptibly in the dark, is heated to the 635th degree, in a pro-longation of Fahrenheit's scale; that it just ceased to shine in

the twilight, when it was heated to the 884th degree ; and that, when it was as red hot as a common fire could make it, the degree of its heat was 1049 or 1050.

To conceive, with some distinctness, the relations which these intense heats have to the temperatures with which we are more familiarly acquainted, it is by no means enough to attend to the numbers which express them. This gives but a very imperfect notion of the comparative magnitude of their intervals. We must have recourse to a scale, or straight line, divided into a great number of equal parts, or units, and place the physical character, or effect of each temperature, at the corresponding units of this scale. By looking at such a figure, we perceive at once, that the difference between the heats of boiling oil of turpentine, and boiling water, is nearly twice as great as that between the heats of boiling and of freezing water; and that this last difference is nearly the same with that of boiling mercury and melting tin, &c. The inspection of such a scale shews us how very small a part of the scale of mea-surable heats comprehends all the temperatures of the habit-able climates, and those of our most familiar and interesting experiments.

But the temperature of glowing iron is far from being the highest of those of many chemical operations, with which it is necessary to have a sort of practical acquaintance. Some metals are observed by those who manufacture them, to melt when red hot, some before, and some after they are red hot. The cutlers have familiar marks when steel and iron are fit for certain operations, and have terms which express those states, such as *cherry red*, *worm red*, *a white heat*, *a blue white*, &c.

You will readily perceive that many of the temperatures in the upper part of the scale are such as cannot be determined by the expansion of a thermometer. They have been deter-mined in Newton's way, (indeed by Newton himself) already described. Some of them have been determined by Mr. Irvin, at my desire, by means of the heat communicated to water, by the lump of iron, immediately after the freezing of the metal. Some of Newton's determinations were by his lintseed oil thermometer. With respect to the temperatures

far below frost, I have paid no regard to those low stations of the mercurial thermometer described by Professor Braun. These were undoubtedly owing to the irregular contractions of portions of the mercury, as they froze. But as the spirit thermometer pointed at the same time to....148°, I have considered this as the temperature of the experiment. For the same reason, I have admitted....120° as a natural cold, once observed in Siberia.

TABLE OF SENSIBLE HEATS.

1050 Iron, as hot as a common fire could make it.
 884 Iron red, in the twilight.
 752 Iron bright red, in the dark.
 635 Lowest ignition of iron, in the dark.
 600 Mercury, and also ol. lini, boil (644 according to Mr. De Se-
     condat.)
 560 Ol. Terebinth boils.
 546 Sulphuric acid boils.
 540 Lead melts (Newton) 585 (Secondat) 594 (Irvin).
 460 Bismuth melts. 476 (Irvin.) also tin 1 pt. and lead 4 pts.
 408 Tin melts. 413 Irvin.
 334 Tin, 3 pts. lead, 2 pts. melt. Also tin, 2 pts. bism. 1 pt.
 283 Tin, 1 pt. bism. 1 pt.
 242 Nitric acid boils.
 212 Water boils. Bism. 5 pts. tin 3 pts. and lead 2 pts. melt.
 174 Alcohol boils.
 156 Serum and Albumen ovi coagulate.
 142 Bees wax melts, Irvin.
 107 Feverish heat.
 100 to 96, Animal heat, or blood-warm.
  64 Summer-heat in this climate.
  32 Frost.
  30 Milk freezes.
  28 Vinegar freezes.
  25 Blood (human) freezes.
  20 Strong wines freeze.
   7 Spirit of wine 1 pt. mixed with 3 pts. water, freezes.
   0 to 0, or from o to—4, salt and snow melt.
 —7 Brandy, or equal pts. of alcohol and water, freezes.
—11 Spirit of wine, 2 pts. mixed with 1 pt. water, (Reaumur).
—14 Cold, observed in the air at Glasgow observatory, anno 1780.
—34 Reaumur's spirit of wine thermometers at Torneao.
—40 Fahrenheit's experiment, and congelation of mercury: also
     nitrous acid crystallizes

—69 Cold produced by Mr. M'Nab, at Albany Fort in Hudson's
    Bay, in December 1784, by means of sulphuric acid and snow.
—121 Siberian cold   } observed with a spirit of wine thermometer.
—148 Braun's expt.   }

1049 or 1050, therefore, is the greatest heat which Sir
Isaac Newton attempted to measure, so as to learn how far
it exceeds other heats, the strength of which is more easily
ascertained.

But the late Mr. Wedgewood, who published two papers
in the philosophical transactions, on the measurement of vio-
lent heats, has gone much farther, and has shewn great in-
genuity in the contrivance of a method for comparing and
distinguishing violent heats.

The principal mean which he employed is the contraction
of the purest kind of clay in the fire.

The properties of the more pure and perfect kinds of clay,
with respect to heat, are well known to be, 1st, a power to re-
sist the most intense heat, without melting. 2d, A disposi-
tion to contract, in all its dimensions, when it is heated
strongly; which contraction is always greater, the greater
the heat is to which the clay is exposed.

This singular property of fine clay, at first, makes it appear
an exception to the general law of expansion of bodies by heat,
and contraction by cold. In reality, however, it is not an
exception; for the general rule of expansion by heat, and con-
traction by cold, is only applicable to bodies which suffer no
permanent change in their composition or texture, by the ac-
tion of heat. But a mass of dry clay does suffer a change in
its texture by the action of a strong heat; it becomes more
compact and hard. I once supposed that it suffered a change
in its composition, by evaporation of its volatile parts; but
Mr. Wedgewood has shewn that this only happens in the be-
ginning of ignition.

The contraction which happens afterwards must be only
from a change of texture, a closer arrangement and contact of
parts, which is permanent, and gives great hardness. Query,
how does heat produce this change of texture? Heat may be
supposed to produce it, by softening the clay, and thus dispo-

sing the particles of it to enter, by their attraction, into closer contact. A mass of clay, simply dried, or made simply red hot, is exceedingly porous, and imbibes water remarkably. The particles of it have not a close contact or connection with one another. It may be compared to dry clay in loose powder, which we know becomes far more compact and dense, by being softened with water, and then allowing it to dry again. *(See note* 11, *at the end of the volume.)*

In whatever manner heat increases the density and hardness of clay, this effect of the heat is permanent, and remains after the heat is gone again ; and a mass of clay, thus changed, is never more affected by any inferior heats in a different manner from other bodies. In these inferior heats, it is expanded, and by cold contracted, like other bodies. It is only by exposing it to a more violent heat than it has suffered before, that any further permanent contraction can be produced.

Of this property, therefore, Mr. Wedgewood takes advantage, in his method of measuring violent heats.

He forms a chosen, and very pure clay, into small, short cylinders, which, by means of instruments properly contrived, are made exactly of the same size. The cylinders are then baked with a low red heat, to expel the air and water, which are contained in raw clay. Being thus prepared, they are ready for the measurement of strong heats, of which they will give information in a few minutes. That time is sufficient for penetrating them thoroughly with the heat to which they are exposed ; and if they be then taken out and cooled, they are always found contracted, more or less, in their dimensions, according to the intensity of the heat to which they were exposed. This contraction being but a very small quantity, the contrivance of a micrometer is necessary for subdividing it. To accomplish this, the clay piece is introduced between two brass rulers, firmly screwed to a plate, and set, with a very small inclination, to each other. Suppose them $\frac{1}{10}$ of an inch farther asunder at one end than at the other, and that they just admit the clay cylinder, after its due preparation, at the wide

end: and suppose that the clay-piece has been in a pottery furnace, and that it is now admitted to the 75th division of the rulers (the whole length being divided into 100 equal parts): we learn, by this experiment, that the cylinder has contracted $\frac{75}{100}$ of $\frac{1}{10}$ of an inch, or has contracted $\frac{75}{1000}$ of an inch, or $\frac{3}{40}$ of an inch.

Mr. Wedgewood's peculiar views made him less solicitous about the inferior heats, and he resolved to begin this scale at the temperature called a full red heat. In order to give numbers to his scale, which should convey some familiar ideas, he took the melting heat of fine gold for a fixed temperature, and by continual bisection, divided it into 32 parts, which he made the degrees of his scale.

By this scale he ascertained the temperature of several chemical processes of great importance in the arts and manufactures.

The highest of these extended to 240° of his scale of heat.

But this gives us little information as to the relation which those increments of temperature bear to those which we have ascertained by means of the common thermometer, even supposing the temperature of full red heat, (the o of Mr. Wedgewood's scale) to have been well determined, which is a matter of some uncertainty. We do not know, from the process now mentioned, how many degrees of Fahrenheit's scale correspond to one of Wedgewood's.

Mr. Wedgewood connected his scale with Fahrenheit's in the following manner: the heat which raised Fahrenheit's thermometer from 50° to 212°, expanded a piece of silver from 0° to 8°, of a certain scale. A heat, which expanded this piece of silver from 0° to 66° of the silver scale, corresponded to 2¹ of the clay or Wedgewood's scale, and 92° of the silver scale corresponded to 6½ of Wedgewood's scale. Therefore, let the lines F f, S s, W w, parallel to each other, represent Fahrenheit's the silver, and Wedgewood's scale. Fahrenheit's scale being divided into its degrees, draw through 50 and 212 the horizontal lines, cutting S s in 0°, and in 8°. Dividing 0-8 into eight equal parts, and carrying forward those divisions, draw lines through 66 and 92, cutting the two lines F f and W w. In Fahrenheit's scale, these lines will fall on 1386 and 1913, and on Wedgewook's scale they give 2½ and 6½ from which we readily find the place 0 to correspond to 1090 of Fahrenheit. Mr. Wedgewood means his scale to commence with the heat given to iron by a strong fire, and reckons this 1077

	f	s	w
1913	----	92	6¼
1386	----	66	2¼
0190	---	----	0
212	----	8	
50	---	0	
0			
	F	S	W

of Fahrenheit, differing from this construction about $\frac{1}{16}$ of his degree, which is nearly 13° of Fahrenheit. (See Phil. Trans. vol. 72.) But I am by no means certain that the contractions of clay are proportional to the heats; but although they are not, this ingenious contrivance is still fitted for giving us very useful information concerning those intense heats. Such heats certainly induce chemical changes; such as tendencies to vitrification, or even this contraction observed in the purest clay, which may effect the heat communicated by them to water, which was the

only other method which promised much accuracy in determining their temperatures.

SCALE OF WEDGEWOOD'S THERMOMETER.

FAHRENHEIT'S.

Extremity of the scale, or highest temperature observed,	240	32277
Greatest heat in an air-furnace eight inches square,	160	21877
Cast-iron melts,	130	17977
Smith's forge hottest,	125	17327
Welding heat of iron,	95	13427
to	90	12777
Fine gold melts,	32	5237
Fine silver,	28	4717
Swedish copper,	27	4587
Brass,	21	3807
Enamel colours,	6	1857
Red heat fully visible in day-light,	0	1077
Red heat fully visible in the dark,	0	947
Mercury boils,		600
Water boils,	0	212

To these may be added some other temperatures, which are interesting to many classes of inquirers:

Hessian crucible melted in an iron furnace at	150	20577
Soft iron nails melted with the bottom of the crucible,	154	21097
(The parts untouched by the nails remained unaltered)		
Flint-glass furnace strongest heat,	114	15897
Another do. do.	70	10177
Plate-glass strongest heat,	124	16807
Settling heat, 28 (4717) or	29	4847
Working do.	57	8480
Delft-ware fired, 40 (6277) or	41	6507
Cream-coloured,	86	12257
Stone-ware, or Pots de Gres,	102	14337
Worcester china vitrified, at	94	13297
Mr. Sprimont's Chelsea china,	105	14727
Derby do.	112	15637
Bow do.	121	16007
Bristol china, no vitrification, at	135	18627

Common Chinese do. no vitrification with any heat, but softened and sunk down at 130°.

True stone nankeen continues bibulous to the last.

Dresden is more refractory than common china. Cream-colored or queen's-ware bears heat as well as the Dresden.

To melt equal parts of chalk and clay, which Pott says is a master-piece of art, requires 123. -

From a general comparison of all these observations, we see that the greatest range of observations which have been made by means of Fahrenheit's thermometer, does not exceed $\frac{1}{32}$ of that of the observations made by Mr. Wedgewood, and which daily occur in the practice of the arts. It is also evident, that by a prosecution of such observations, much valuable instruction may be obtained.

SECT. V.....OF INFLAMMATION.

THIS is the phenomenon familiarly known by the name of BURNING, or COMBUSTION. It obtains in such bodies as take fire, or are kindled in a part, by the application of a very hot body, or otherwise, and continue to give out light and heat to other bodies, till they are burnt all over, or their faculty of burning is exhausted, which we express in common language by saying that they are consumed.

Seeing that all bodies will not kindle and burn, as is well known, it may be thought improper to include this among the general effects of heat. But it is very general, and happens in many cases where we do not attend to it, such as lead, iron, and other metals; nay, even in the diamond. But my chief reason is, that an acquaintance with the distinguishing phenomena of combustion, is indispensably necessary for our farther progress.

With these views, I observe, in the first place, that no natural operations, nor any experiments, shew us that those substances which do not burn, either suffer any permanent changes in their principal qualities, as subjects of chemical examination, by being thus heated, or afterwards give out more heat than they had received. They readily receive heat, and transmit it to others; but without either

increasing or diminishing the quantity of that heat, so far as we can perceive, or being themselves altered in their principal qualities, in consequence of having received it and lost it again. Thus, water, or any pure saline or earthy substance, readily receives any quantity of heat to which it is exposed. It will be expanded, perhaps may suffer a change from the state of a solid to that of a fluid, or from the state of a fluid or solid to that of vapour. Or, if it be a fixed substance, it may be ignited, and will preserve its new form and appearance as long as the heat continues; but, if it be placed beside a colder body, it will quickly lose its superiority of heat, and, at the same time, return to its former condition in other respects. The vapour of water or salt, returns again to water or salt. And a salt or earth, after having received so much heat as to be ignited, or perhaps melted, returns, in cooling, to the same state as before; at least it is not altered in its principal properties. It is still a salt, or an earthy substance, while, at the same time, the heat which it had received, is transmitted to other bodies, without increase or diminition, that can be distinctly observed.

But such is the nature of inflammable substances, that they are different from others in these two particulars. For, if they be heated to a proper degree in the open air, they not only continue hot, but become hotter; and this, in some circumstances, to a violent degree of intensity. And they continue in this state, called the state of INFLAMMATION, for a longer or shorter time, during which they send out of them, into the surrounding matter, a continued, and often very copious, stream of heat and light, the whole of which, it is plain, they have not received in the usual manner, or by communication from other bodies.

Further, this flow of heat comes to an end at last, and then we find that the inflammable body has undergone a most remarkable change. It has lost the qualities by which it was distinguished as an inflammable body. It cannot now be again made a scource of heat, nor is it qualified any longer to exhibit the phenomena I have described, but is on the same footing with other uninflammable matter, with respect to heat.

Such is, therefore, the general nature of the inflammable substances, and the distinction between them and other bodies. There are some of them which, to a careless observer, may seem, in some respects, exceptions to this general account; which, during inflammation, instead of being changed into a new sort of matter, appear to be totally spent and consumed, so that nothing remains in their place. Such is spirit of wine. Dr. Boerhaave considers it as a pure pabulum of fire. Such also is sulphur. But the appearances here are deceitful. Spirit of wine is changed by inflammation into a great quantity of water....Sulphur into a great quantity of acid.

The matters produced from the other inflammable substances are very various. From phosphorus we obtain, by inflammation, a fixed acid; from others we obtain water, earths, salts of different kinds, and aerial matter; and some afford several of these substances at the same time. This is the case of the common animal and vegetable inflammable substances.

All the inflammable substances, therefore, are changed, during their inflammation, into one or more principles, or kinds of matter, which are no longer inflammable.

Another particular, which must be attended to in considering the general nature of inflammation, and of the inflammable bodies, is the necessity of air to inflammation.

I began this subject with saying that the inflammable substances enter into a state of inflammation, and suffer the change described, when heated to a certain degree in the *open air*. The being heated to a certain degree, in the first place, is a circumstance undoubtedly necessary to the beginning of inflammation, though the degree or intensity of heat necessary is very different, in the different kinds of them. More or less heat, however, is necessary in all cases; and I have chosen, therefore, to introduce this subject of inflammation among the effects of heat.

But it is equally necessary that the body be exposed to the action of air, and even of fresh air, constantly renewed at its surface. If we attempt to inflame a body in a space exhaust

ed of air, we never succeed. If we even attempt it, in air that is confined, the inflammation soon stops, although the inflammability of the burning body be not exhausted. The air with which it was inclosed undergoes a change, which renders it unfit any longer to continue the inflammation which it promoted at first*.

These are the chief facts relating to this subject, and which have been remarked for a long time. It will be proper, now, to attend a little to the opinions which have been formed concerning the nature and causes of these phenomena.

The opinion which was first formed among the chemists, and was received as just and adequate for a considerable time, was this:

It was imagined that the quality of inflammability depended on a principle, or material substance, which they supposed to be contained in all the inflammable substances, and to be the same in all the different kinds of them; and that the diversity among them, in external appearance, and other qualities, proceeded from the other principle, or number of principles, with which this common principle is combined; and the chemists considered inflammation, with the several phenomena that attend it, as depending on a gradual separation and dissipation of this common principle, the φλογιστον, which being once separated, what remained of the body could no longer be an inflammable substance, but must be similar to the other kinds of matter.

This opinion was founded chiefly on some facts and experiments, of which it is proper to give you a general knowledge. There are numerous experiments and operations in chemistry, whereby the uninflammable matter, into which some of these bodies are changed during their inflammation, can be restored again to its former state, and rendered again

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* There are, indeed, a few cases where bodies will burn, and be consumed, in the closest vessels. This you all know to happen with gunpowder, and all substances containing saltpetre, and any ordinary combustible. But chemistry teaches us that this is no exception, and that the saltpetre is decomposed by the heat and the combustible body, and yields a continual supply of the purest air. EDITOR.

Phlogiston

capable of being inflamed. We have noted examples of this
in sulphur and in phosphorus. Both of these inflammable
substances are changed, by inflammation, into acids, the one
of which is called the sulphuric acid, or vitriolic acid, and
other the phosphoric acid. But each of these acids can be
again restored to the state of an inflammable body; the one
to the state of sulphur, the other to that of phosphorus, as
before: and this restoration is effected by mixing them with
charcoal dust, and exposing this mixture to a strong heat.
The charcoal employed, or a part of it, is consumed, and dis-
appears, while the acid resumes the form of an inflammable
body. The disappearance or diminution of the charcoal in
these operations, made the chemists imagine that a part of it
was combined with the acid matter, and brought it back to the
state of an inflammable substance, by restoring to it some prin-
ciple which it had lost during its own production from the
inflammable substance, by inflammation.

This supposition having been formed, certain variations of
these experiments were thought to prove that this supposed
principle is the same in all the inflammable bodies; for, in
restoring the acid of sulphur to the state of sulphur again, or
the acid of phosphorus to the state of phosphorus, we are not
limited to the use of any one kind of charcoal, or indeed any
one kind of combustible matter, if it be sufficiently fixed
to bear the necessary heat, without escaping in volatile fumes.

Upon these facts, therefore, was founded a persuasion that
the inflammable substances contain a principle which gives
them the quality of inflammability, which they have in com-
mon, and which principle the chemists called phlogiston, as-
serting that it is precisely the same in all the species belong-
ing to this class, and that it is dissipated from them during
their inflammation, but can be again restored by processes
like those I have just now described.

But when we inquire further, and endeavor to learn what
notion was formed of the nature of this principle, and what
qualities it was supposed to have in its separate state, we find
this part of the subject very obscure and unsatisfactory, and
the opinions very unsettled.

The elder chemists, and the alchemists, considered sulphur as the universal inflammable principle, or at least they chose to call the inflammable part of all bodies, that are more or less inflammable, by the name of their sulphur, which they appear to have considered as a general principle of inflammability. The famous German chemist, Becher, was, I believe, the first who rejected the notion of sulphur being the principle of inflammability in bodies. He considered sulphur as one of the inflammable substances, which contained the principle of inflammability like the rest, but which was not the pure principle of inflammability itself.

His notion of the nature of the pure principle of inflammability was afterwards more fully explained and supported by Professor Stahl, who, agreeably to the doctrine of Becher, represented the principle of inflammability as a dry substance, or of an earthy nature, the particles of which were exquisitely subtile, and were much disposed to be agitated, and set in motion with inconceivable velocity. You have a full account of this explanation in his *Theoria Chemiæ Dogmaticæ*, a work of great merit and ingenuity, and which was long regarded as the infallible code of chemical science.

The opinion of Becher and Stahl concerning their *Terra Secunda*, or *Terra Inflammubilis*, or *Phlogiston*, was, that the atoms of it are, more than all others, disposed to be affected with an excessively swift whirling motion, *motus vorticillaris*. The particles of other elementary substances are likewise liable to be affected with the same sort of motion, but not so liable as these of the *Terra Secunda:* and, when the particles of any body are agitated with this sort of motion, the body exhibits the phenomena of heat, or ignition, or inflammation, according to the violence and rapidity of the motion. This notion of heat, as depending on a motion or agitation induced in the particles of bodies, was founded on the common observations and conclusions, first inferred by Lord Verulam, from the consideration of the different means by which heat may be produced or excited in bodies. Becher and Stahl, therefore, did not suppose that heat depended on the abundance of a peculiar

matter, such as the matter of heat or fire is now supposed to be, but on a peculiar motion of the particles of matter; and that their phlogiston, which was supposed to be contained in all the inflammable substances, was, most of all other matter, disposed to assume this sort of motion. *Vide Juncker, vol. 1. Tab. de Principiis, et Tab. de Instrumentis Chemiæ essentialibus.*

This very crude opinion of the earthy nature of the principle of inflammability appears to have been deduced from a quality of many of the inflammable substances, by which they resist the action of water as a solvent. The greater number of the earthy substances are little, or not at all, soluble in water; and some time ago they were reckoned, when in their pure state, perfectly insoluble. And, as Becher and Stahl found those compounds, which they supposed contained phlogiston in the largest quantity, to be insoluble in water, although the other matter, with which the phlogiston was supposed to be united, was, in its seperate state, exceedingly soluble in that fluid, they concluded that *a dry nature, an incapability to be combined with water*, was an eminent quality of their phlogiston; and this was what they meant by calling it an earth, or an earthy substance.

They thought it was most abundant, and least clogged with matter in charcoal, and in soot, both of which are very inflammable, and insoluble in water, and, in some measure, shew even a repulsion for that fluid. The finest kind of soot (lamp black) is very difficultly wetted with water.

But these authors supposed, at the same time, that the particles of this dry and earthy phlogiston were much disposed to be excessively agitated with a whirling motion; which whirling motion, exerted in all directions from the bodies in which phlogiston is contained, produced the phenomena of inflammation.

This appears to have been the notion formed by Becher and by Stahl, concerning the nature of the principle of inflammability, or the phlogiston; a notion which seems the least entitled to the name of explanation, of any thing we can think of. I presume that few persons can form any clear conception of this whirling motion, or, if they

can, are able to explain to themselves, how it produces, or can produce, any thing like the phenomena of heat or fire.

Succeeding chemists, however, refined afterwards upon this notion, and formed an opinion which had much more appearance of probability.

This opinion was founded in the phenomena of inflammation, and, and on some new facts relating to this subject, which Becher and Stahl had not attended to, or had not the opportunity of observing.

When a combustible body is set on fire, and is undergoing the change which I have described what appears the most obviously, and evidently, to be separated from it, and to be thrown off, is heat and light. But, from a great number of considerations, there is reason to conclude, that heat and light depend on the motion and action of an inconceivably subtile, penetrating, and elastic matter, the particles of which are minute beyond what the utmost efforts of our imagination can conceive, and have a strong repulsion for one another ; and thus are capable of being thrown off from bodies, or set in motion, with a velocity that is astonishing. It has been proved; by astronomical observations, that light comes from the sun to us in 8'. 13''. A cannon ball, flying with the velocity of 600 feet per second, would require more than 25 years to go to the same distance. The velocity of light, therefore, is perfectly astonishing, and would enable it to beat into powder, and disperse in dust, every object on which it shines, were not the particles of it so inconceivable fine and minute, that they are thereby deprived of this destructive power. Their minuteness is such, that some have demonstrated, that 200,000,000,000 of them, at least, can pass through a pinhole made in a card, crossing one another in every direction, and in the same moment of time, or at least in a time so short, that it appears indivisible.

This exquisitely subtile and active matter, therefore, was, by the later chemists, supposed to be capable of being fixed and combined with other matter, so as to enter into the composition of certain bodies ; and, in this combined and fixed state, gave to such bodies the quality of

inflammability, and was separated from them during their inflammation.

This is the opinion I had of it formerly, before the existence of a phlogiston, or principle of inflammability, began to be doubted. Mr. Macquer, and other chemists also formed the same opinion.

One principal foundation of this opinion was a number of experiments and observations, from which it appears that certain bodies, not inflammable, are changed into inflammable bodies, by simply exposing them to the light of the sun ; or, if they be not actually changed into inflammable bodies, they suffer the same changes as those they suffer, when we apply to them other inflammable substances, with the intention to communicate to them the quality of inflammability, or other qualities which are connected with it.

We have an example of this, in the production of the green matter of the leaves of plants. It has long been known, that the green colour of the leaves of vegetables is produced by the light of the sun. The most direct experiments to ascertain this fact, were made first by Mr. Dufay, and others of the French academicians ; and the investigation has been since carried farther by Mr. Sennebier of Geneva, and others. When seeds are sown in a dark place, the plant grows luxuriantly, but with a foliage extremely different from what is common to it, and entirely without the green colour. This is the case, although plenty of fresh air be admitted to them. When the plant is in this state, if the light be admitted, it immediately begins to acquire a green colour, which in time arrives at its ordinary fulness. What is farther remarkable in this experiment is, that the plant directs its growth to the light, although the hole by which this is admitted be closed up by a piece of glass.

Professor Robinson informed me, that he saw tansy growing in a coal-pit, with a very abundant foliage, but white, and without smell ; and that, when the clod was brought above ground, this subterranean growth died down, and was succeeded by fresh leaves of a full green, and strong aromatic smell. Hence he concluded, that the sun's rays not only produce the green fæcula, but also

the essential oil of plants. Hence we may account for the deep green, and the great abundance of aromatic plants, shrubs, and trees, in the warm climates. *(See note 12, at the end of the volume.)* Upon this depends the art of blanching endive, celery, and many other garden plants, for the use of the table, in order to have them tender and sweet, and free from any strong biting taste. We bind up their leaves as they sprout, keeping them from the light. Some esculent vegetables produce such a tuft of leaves, and these so broad and compacted, that the external leaves alone get the benefit of the light, and are colored green by it. Such vegetables, therefore, scarcely require the aid of art to blanch them. Such are some kinds of lettuce and cabbage. It is quite plain, therefore, that the green colour of their leaves is produced by light. A long time ago, I had the curiosity to examine this green matter, to learn the nature of it. It separates from the rest of the matter of the plant, when that is mashed and macerated with water. It may be completely taken up by spirit of wine. This fluid dissolves the green fæcula completely, and may afterwards be expelled from it by heat. It is highly inflammable, and burns completely, without leaving any sensible quantity of ashes.

Thus have I briefly stated the opinions concerning the nature of inflammation, and of the inflammable bodies, which had been formed among the chemists until about fifteen or twenty years ago. Before that time, however, a fact had been observed with surprise that was very inconsistent with this opinion, viz. That some of the inflammable bodies, when changed by inflammation into uninflammable matter, afford a quantity of this matter of greater weight than the inflammable body itself. About the period I just now mentioned, or since that period, this fact has been better ascertained, and discovered to be true with respect to all the inflammable bodies, and in all cases of inflammation. The reason why the chemists were so late in discovering this fact, with respect to a great number of these cases, was, that the uninflammable matter produced is, in many of the experiments, a volatile substance, and escaped during the examination. But, ingenious methods

having been devised for collecting and weighing the whole
of this volatile matter, as well as the rest of the uninflam-
mable substances into which bodies are changed during the
inflammation, it has been found, that in general, there is
more of this uninflammable matter than there was of the
body before it was inflamed. From an ounce of phos-
phorus, for example, we may perhaps get an ounce and a
a half, or more, of the solid acid salt, into which it is
changed by being inflamed.

This appeared to many an insuperable objection to the
theory of the chemists ; and, though some attempts were
made to obviate this objection, they rested on suppositions
so very subtile and difficulty comprehended, perhaps absurd,
that they were quite unsuccessful.

Another theory, therefore, which I may say is the oppo-
site or contrary to this, has lately been formed, and is fast
gaining ground, especially on the continent. I cannot ex-
plain to you at present the whole of this theory, or trace
the steps by which it has been brought to its present state.
We shall have better opportunities for doing this in other
parts of the course, as you will have by that time become
well acquainted with several substances, whose properties
have a great influence in producing the phenomena which
furnish the chief arguments in support of the theory. I
can only remark just now, that it is founded on a very
great number of experiments and discoveries, which have
been made within these twenty years (1789) on the nature
and qualities of atmospherical air, and of a number of other
elastic fluids.

Ingenious men in different parts of Europe, incited by
the remarkable properties which I had discovered in one
of those fluids in 1756, began to employ themselves in such
experiments, and they have made many astonishing and
most interesting discoveries. The fundamental experi-
ments were first made, and the leading inferences were
first drawn in this country, by Dr. Priestley, the Honor-
able Mr. Cavendish, and my friend Mr. Watt. But it was
chiefly in France that they were repeated, with proper
attention to all the circumstances that would affect the
result, and this result was made the foundation of a new

theory of combustion. These experiments have been
made there with extraordinary ingenuity and accuracy,
and on so large a scale, that the effects produced were
the more clearly and fully perceived. The unfortunate
Lavoisier, who fell a sacrifice to the ambition of his phil-
osophical associates, and whose unjust and cruel fate, and
the loss which science has suffered by it, cannot be too
much deplored, was the most active in this inquiry, spar-
ing neither labour nor expence. His own exertions dis-
play the greatest ingenuity. He also employed the talents
of other eminent philosophers in ascertaining some of the
most important and interesting facts ; but Lavoisier him-
self was the principal author of the new theory.

It had long before been known, as I have already in-
formed you, that air, and even a constant supply of fresh
air, is necessary to inflammation; that if a burning body
be shut up with a certain moderate quantity only of air,
the inflammation stops after some time, although the in-
flammability of the body be not exhausted. The inclosed
air has undergone a change which makes it totally unfit
for continuing the inflammation. In some of the experi-
ments made by Dr. Hales, and other persons, it had also
appeared, that the air is generally diminished on this oc-
casion, or, in the language of Dr. Hales, a part of it was
absorbed.

But this has been ascertained with much more preci-
sion, and in a much greater number of cases, by the later
experiments. It has become clear and evident, by many
of these, that a considerable quantity of the air is really
absorbed, and combined with the matter of the burning
body, so as to form, in many cases, a dense compound,
in which the air so absorbed is totally deprived of its usual
form of an elastic fluid: and the additional weight which
the matter of the burning body acquires, has been found
to correspond exactly to the weight of the air which has
been absorbed by it.

Farther, the two very different combinations of heat,
which I had discovered, in which it produces fluidity and
vapour, encouraged Mr. Lavoisier to presume that there

was another combination, which produced a permanently elastic fluid, not decompoundable, like a liquid, or a vapour, by the touch of a colder body*; and the different capacities for heat, having been already discovered, it appeared no difficulty to account for the vast quantities set loose in combustion. This new theory, therefore, is founded on the doctrine of latent heat, and is, indeed, an extension of it.

For, in the remarks I lately made to you on the production of vapour from bodies by the action of heat, I made it evident, that vapour contains a very great quantity of heat, beside that which we perceive in it by the simple application of the thermometer; and that this heat, which appears to be a necessary ingredient in the composition of the vapour, comes out of it again when it is condensed. In Mr. Lavoisier's theory, the same thing is supposed to happen during the action of air and burning bodies on one another. The matter of those bodies attracts the matter, or basis of the air, or rather of a part of the air, which is different from the rest, and with which I shall make you hereafter better acquainted. The burning body collects, and condenses this sort of air, depriving it at once of its vaporous and elastic aerial form; or, if it does not absolutely condense it to a solid matter, it diminishes, to a great degree, its capacity for heat. All, or the greater part, of that great quantity of heat, therefore, which was combined with the matter of this air, and thermometrically latent in it, is suddenly let loose, to diffuse itself among the surrounding bodies: and there is a continued emission of it, in this manner, so long as the air and burning body continue to act on one another, or until that portion of the air, which is capable of being thus attracted, and compounded with the burning body, is totally expended. This theory, therefore, is directly contrary to the theory the chemists had formed before. *(See note* 13, *at the end of the volume.)*

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* This is not strictly true, for the oxygenated muriatic acid gas is condensed by a cold....32o of Fahrenheit.

According to this theory, the inflammable bodies, sulphur, for example, or phosphorus, are simple substances. The acid into which they are changed by inflammation is a compound. The chemists, on the contrary, considered the inflammable bodies as compounds, and the uninflammable matter as more simple. In the common theory, the heat and light are supposed to emanate from, or to be furnished by, the burning body. But, in Mr. Lavoisier's theory, both are held to be furnished by the air, of which they are held to be constituent parts, or ingredients, while in its state of fire-supporting air.

Such are the outlines of this celebrated theory of combustion. I cannot enter on it in greater detail, in this part of the course, but this brief sketch was necessary for the understanding what follows concerning the management of fuel in our furnaces, and for some other purposes. The full consideration of it will be given, when those fuels are considered as subjects of chemical examination, by the help of doctrines depending on the chemical nature of other substances, of which the treatment is more elementary.

After these observations on the general process and phenomena of combustion I must explain to you some appearances which, though not universal, yet obtain to a very great extent, and occasion important differences in the phenomena, and in the employment of combustion, as an instrument of chemistry.

FLAME, and that particular form of combustion which is properly called INFLAMMATION, requires some particular consideration. With this view, therefore, I must first remark that different inflammable substances have very different degrees of volatility. Some cannot be converted into vapour with any degree of heat which we can command. These do not suffer any loss from heat, if we preserve them from the action of the air; and when the air is admitted, their combustion is attended with a gradual consumption of their surface only. But many others are more or less volatile, and many so volatile, that they begin to be converted into vapour before we can heat them to the degree necessary for their inflammation. It is evident that we cannot inflame these, except in

the form of vapour;.....they are more volatile than inflamma-ble. The usual practice, therefore, is to heat them, or a small part of them, until they emit vapour, and then, at the same time, to set that vapour on fire. Thus we produce FLAME, the heat of which occasions more vapour to rise, and to be inflamed, in a constant succession, until the whole mass has undergone inflammation. This is illustrated by examin-ing attentively the flame of spirits of wine, burning in a small vessel or dish, $1\frac{1}{2}$ or 2 inches diameter. *(See note* 14, *at the end of the volume.)*

That the flame, even of oily, substances, as oil, or tallow, is truly a transparent vapour, which appears otherwise in a dark place, in consequence of its emitting heat and light, becomes evident, when we place a candle in the sunshine. The light of it being too weak to be perceived in the strong light of the sun, the flame is not seen. We see objects placed beyond it, as well as if it were not interposed; and when we observe the shadow of it on a wall, we see no shadow but that of the wick; the rays of the sun pass through the flame, and reach the wall without interruption. They are only a little refracted, bent from their straight course, while they pass through the rare vapour of which the flame is composed, or through the air heated by the flame; and therefore the shadow of the candle, when thrown on a white wall, shews an appearance of thin smoke arising from the wick.

GENERAL REFLECTIONS.

I have now finished the account I proposed to give of the more general effects of heat; in communicating which, I have followed the plan which appears to me the most proper to lead to improvements in our knowledge of this subject, and by following which, many improvements have actually been made.

One part of the subject, however, may appear to you to have been omitted; and it is a part upon which Dr. Boer-haave has bestowed a great deal of labour and study. The part I now refer to is the different means by which heat may

be produced, or increased, and even diminished also; or the different means for producing heat, or cold.

But although I have not attempted, as Dr. Boerhaave has done, to bring together all the facts belonging to this part of the subject, I have taken occasion to mention the greater number, and the principal of those facts. To have treated them as minutely as that author, would have taken up too much of our time at present; and we shall have a better opportunity hereafter to notice those parts of it which remain untouched. But, from the progress we have already made in studying the general effects of heat, I am persuaded you will view the subject as one of the most curious and interesting parts of the study of nature.

There is, perhaps, no inquiry more worthy of the attention of the philosopher than the nature of heat, and the manner in which matter in general, and the different kinds of it, are affected by this wonderful agent. Its influence is manifest, so universal, and its action so important and necessary to the progress of all the operations of nature, that, to those who consider it with some attention, it will appear to be the general material principle of all motion, activity, and life, in this globe. Heat is inseparably necessary to the very existence of vegetables and animals. Without heat, plants cannot live. Without it, they want the power to attract their nourishment, or to set it in motion through their system, or to refine and ripen it in their different parts. Their vigour and life depend on its influence. It is only when enlivened by heat, that they make it assume the various forms and qualities which we find in the wood, the root, the leaves, the juices, the fruit, the seeds; and the beautiful forms and colours displayed in the flowers. They decay and die when heat departs. Nor is animal life less immediately dependent on heat, for support, than vegetable. Heat is the main spring in the corporeal part of an animal, without which all motion and life would instantly stop. There are few facts more unaccountable than the effect of heat on an egg, though there are few to which we pay so little attention. We see a lump of apparently dead matter, which, left to itself, would continue dead and inactive for ever. By the application of a gentle degree

of heat, it soon has an animal formed within it, which quickly increases in size and perfection, until it breaks open its inclosure, perfect in all its parts, and ready to perform its proper functions.

The effect of the heat, upon this occasion, does not depend upon the particular manner in which it is produced or applied. In most cases, it is supplied by the mother; but it is well known, that in a very great number, no heat is necessary but that of the sun: and art has succeeded in employing the heat of fuel, and that produced by masses of fermenting or putrefying matter.

In the incubation of an egg, therefore, we see an example of an animal brought into existence by heat, operating as a circumstance necessary or indispensable to its production. But, after the animal is thus brought into existence, heat is still necessary for its support. If heat be diminished to a certain degree, although no visible damage be produced, all motion and life are quickly extinguished. The animal is seized with a sleep and insensibility, under which it expires. This is well known in the countries where the colds of winter are violent; and the inhabitants are upon their guard to shake off the first appearance of drowsiness, produced by excessive cold. There are, it is true, some kinds of animals, as swallows, bats, frogs, &c. to which this sort of sleep is not fatal; that is, it does not destroy the seeds of life so effectually as to take away the possibility of its returning. But even this sleep, when it continues, is attended with all the symptoms of death, except putrefaction, and nothing but the return of heat can restore to the animal its life and activity.

Thus, therefore, we see that the whole of the vegetable creation, and all the varieties of animal life, depend on heat for production and support, or cannot be produced and supported without it. How wonderful and extensive, therefore, in its operation and influence, as confined within limits set to it by the Author of nature, and as subject to the gentle and regular vicissitudes of day and night, summer and winter!

It is plain, that not only all animal and vegetable life, but that the whole face and appearance of nature, the very form and

powers of the elements themselves, depend on this limited action of heat. There are none of the elementary bodies with which we are better acquainted than water. Let us attend a little to the powers and qualities by which it acts its part in this system of beings. We all admire its pure transparency in a spring ; the level and polished surface with which it reflects objects that are on the banks of a lake ; the mobility with which it runs along the channel of a brook, and the incessant motion of its waves in a stormy sea. But, when viewed with a philosophical eye, it appears much more an object of admiration. The same water, which, under its usual form, is such a principal beauty in the scene of nature, is employed in her most extensive operations, and is necessary to the formation of all her productions. It penetrates the interior parts of the earth, and appears to assist in the production of various minerals, stones, and earths, found there, by bringing their different ingredients together, and applying them to one another properly, that they may concrete. We know it arises in vapours from the surface of the ocean, to form the clouds, and to descend again in rain upon the dry land, and give origin to springs, rivers, and lakes ; or, upon proper occasions, to form deep snow, which protects the ground and vegetables from the intense and mortal cold to which some parts of the world are exposed ; and, after it has performed this useful office, it readily yields to the heat of summer, and returns to a state in which it serves the same purposes as rain. By its fluidity and tenuity, it penetrates the soil, and the seeds of plants which that soil contains. These it causes to swell and germinate into plants, which depend on water for support. It passes with freedom and ease through all their minutest tubes and vessels, and carries with it materials necessary for nourishment and growth, or changes its appearance so as to become part of the plant. There is no plant or vegetable substance, that does not contain in its composition a large quantity of water, easily separable from it. The hardest woods contain a great deal. The softer and more succulent parts of vegetables are almost totally composed of it. Even the oils and resinous substances can be resolved in part into water. It

is plainly as necesaary to the animals, and is found to be as copious an ingredient in the composition of their bodies, and of all the different parts of them.

These are the numerous and extensive uses of this beautiful element. But, in this succession of forms and operations which it undergoes, you will perceive that it is set in motion, and adapted to these ends, by the nice adjustment and gentle vicissitudes of heat and cold, which attend the returns of day and night, and summer and winter; and that even the *form*, under which it and the other elements play their parts, depends on the limited action of heat. Were our heat to be diminished, and to continue diminished, to a degree not very far below the ordinary temperature, the water would lose its fluidity, and assume the form of a solid hard body, totally unfit for the numerous purposes which it serves at present. And, if the diminution of heat were to go still farther, the air itself would lose its elasticity, and would be frozen to a solid useless matter like the water; and thus all nature would become a lifeless, silent, and dismal ruin. Such being the important part allotted to water, in the magnificent series of natural operations, in consequence of the qualities communicated to it by heat, all its properties become interesting objects of contemplation to a sensible heart. That peculiarity, by which the expansion and contraction of water by heat is distinguished from the same effect on other substances, I mean its irregularity between 32° and 40° of Fahrenheit, naturally attracts attention. Even this seemingly trifling distinction has been shewn by Count Rumford to have a mighty effect in rendering our habitation more comfortable.

On the other hand, were the heat which at present cherishes and enlivens this globe, allowed to increase beyond the bounds at present prescribed to it; beside the destruction of all animal and vegetable life, which would be the immediate and inevitable consequence, the water would lose its present form, and assume that of an elastic vapour like air; the solid parts of the globe would be melted and confounded together, or mixed with the air and water in smoke and vapour; and nature would return to the original chaos.

Such are the objects and reflections which present themselves to our minds, when we contemplate the general effects of heat in the universe : and with these I now finish the first section of the first part of our course.

PART II.

GENERAL EFFECTS OF MIXTURE.

INFORMED as you now are of the general effects of heat, your minds will be greatly assisted in your endeavors to form distinct conceptions of the results of the mixture of different substances. This, you know, is the other great mean of accomplishing the chemical changes in nature or art.

Upon this subject we have not so many general facts to communicate as on the subject of heat. The effects of mixture are so extremely diversified, that it is more difficult to find such as occur in a great number of cases, or which can be considered as general effects. Yet there are some of this kind ; and it is proper to take notice of them here, that we may have an opportunity to become acquainted with the bond of chemical union, which is the characterestic of our science, and from our notions of which are principally derived all our attempts to explain the chemical phenomena of nature, or to reason concerning them.

It is therefore chiefly with a view to prepare you for understanding the principles by which many of the chemical phenomena are considered as explained, that I am now to lay before you the more general facts relating to mixture ; and they are these :

1. In the first place, we have learned, by innumerable experiments with the various kinds of bodies, that, though some, when mixed together, or applied to one another with a view to mixture, cannot be made to unite ; as oil with water, water with mercury, sand with water ;....others readily unite in the most intimate manner, and form *compounds*, which have, however, all the appearance of being simple and homogeneous, or in which the eye cannot discover, even with a microscope, the smallest signs of any diversity in the constituent parts.

2. In some of these cases again, this intimate union is effected slowly and quietly, and is not accompanied by any other phenomena that are very remarkable ; as, when pure common salt is dissolved in water into a clear brine ; when sugar is dissolved into a syrup, or when vinous spirits are diluted in water. But, in other cases, the subjects unite with an appearance of violence, or with rapidity, commotion, and the production of heat. Of these cases you will have more distinct knowledge by some examples.

First Example. If equal measures of vitriolic acid and water (as eight ounces) be poured into a Florence flask, we observe that the intimate union is performed with rapidity, commotion, and the production of heat. The heat produced from the sudden mixture of this quantity of strong acid and water, is so great, that it will split the glass vessel, unless extremely thin.

Second Example. Another instance may be taken, where the appearance of violence and commotion is still greater, though the heat produced is not so considerable. Put into a tall glass a small quantity (suppose four ounces) of the common spirit of hartshorn, called by the chemists mild volatile alkali, united with water, and pour into it moderately strong vitriolic acid, by small quantities at a time : the liquor instantly boils up, foaming and frothing, and sputtering up many small drops. This turbulent motion ceases after a little while, but is renewed by the next addition of acid. After a few such additions, the foaming becomes less remarkable, but will increase by agitating the vessel, or by stirring the mixture. After some time, no more of it appears. We have now pro-

duced a mixture perfectly transparent and uniform in all its parts.

The appearances of turbulent union, and most violent action of the substances on each other, are still more remarkable in some cases; and even smoke and flame sometimes burst out with violent explosion.

I may take this opportunity to remark, that this sort of violent and turbulent intestine motion, and foaming, which accompany the union of different bodies, is called EFFERVES-CENCE, among the chemists, and that the bodies are said to EFFERVESCE with one another.

3. These examples are cases of the union of fluids with one another. There are many others, which, at the first view, may perhaps appear more surprising, in which even solid, and sometimes very hard bodies, when thrown into certain liquids, begin immediately to be torn asunder, with more or less rapidity and violence, into atoms so minute as to become entirely imperceptible to our senses; in which state of exquisitely subtile division, they are dispersed through every part of the fluid, and compose, with it, an apparently homogeneous liquor. They form, with the fluid, a liquor which is not only quite transparent, and, in appearance, pure and simple at first, but which, though kept for ages, will remain so, without depositing the smallest sediment, or shewing any other signs of its being compounded. This happens, too, in many cases, although the solid be composed of much heavier matter than the fluid, and sink in it, at first, as a stone in water. Notwithstanding its superior weight, it is soon combined with the fluid, in the manner I just now mentioned, and the particles of it never afterwards separate or subside to the bottom, but will remain for ever equally dispersed through every part of the fluid, and closely and intimately united with it.

This happens to different solid bodies, with more or less rapidity, and appearance of violence. As examples, therefore, of such combinations, I shall state one case, in which the solid is divided and dispersed quietly, and one in which its division is accompanied with appearances of violent action.

We shall afterwards, as we go on, have occasion to see ex-
amples of this kind in great abundance. The first example is
the solution of camphor in spirit of wine. In this mixture,
nothing can be observed but the slow and gradual diminution
of the camphor, and the sensible diffusion of its pungent and
hot taste in every part of the liquor. The second is the solu-
tion of a bit of marble in muriatic acid, or spirit of salt. In
this experiment, we find that the bit of marble is no sooner
dropped into the liquor, than the particles of it begin to be se-
parated one from another, and to be dispersed in invisible
atoms through the fluid so that in a short time the marble
totally disappears ; and, while this goes on, there is an effer-
vescence, or appearance of constant agitation, and turbulent
commotion, all round the bit of stone, and innumerable bubbles
arise from it, as if the fluid were boiling. The same effer-
vescence is seen here, though less violent, as that which ap-
peared in the mixture of the vitriolic acid and volatile alkali.

Besides these symptoms of violence, with which certain
bodies unite together, it appears, from other particulars, that
some of them have a very strong propensity to unite. They will
unite under circumstances very unfavorable or when the union
might be thought to be attended with considerable difficulty.
Thus, some substances will collect the water that is in the air
in the form of invisible vapour, and will be dissolved by it,
although the air appear very dry, and contain but little of such
humidity, so that other bodies containing humidity, would
quickly be dried in such air. The vitriolic acid which we
made use of is one example of this. There are other salts
which can be reduced to a dry state, and are usually kept in
that form, but which it is necessary to keep in close vessels,
shut up from communication with the air ; as salt of tartar,
&c. This phenomenon is called DELIQUESCENCE, or running
PER DELIQUIUM ; and the salts which have this disposition
are called DELIQUESCENT SALTS.

To return again to our experiments. We may take notice,
that when a solid is thus attacked by a fluid, thus divided and
dispersed through it so completely and intimately as to
become perfectly fluid along with it, the chemical term for
this change of condition is SOLUTION. A solid body, to

which this happened, is said to be DISSOLVED, and the fluid which has this effect upon it is called the SOLVENT, or MENSTRUUM. This last term is said to have been invented by some of the alchemists as the name of some very powerful solvent, which they made a secret, and which required a month to produce its effect.

I wish you here to attend carefully to this sort of combination of solids with fluids, and to the characters by which it is distinguished; that you may not be misled by inaccuracies in common language, in which the term solution, or dissolution, is often misapplied.

It is not uncommon to say, " That clay dissolves, or can be " dissolved, in water;" because, when put into water, it is first softened, and afterwards reduced to a mud so fine, that, by stirring it well, it can be equally dispersed through the whole of the water. But, in this case, the clay is not intimately *combined* with the water, or reduced along with it to the form of a fluid. The particles of it are only separated from one another, and dispersed. There is no strong and intimate combination, or change of the clay to the form of a fluid, and incorporation with the water. The mixture has not transparancy; it is quite muddy, and, if we allow it to stand at rest for some time, the whole of the clay subsides again to the bottom. The proper name for this kind of mixture, in chemical language, is DIFFUSION. We say, that the clay is DIFFUSED through the water.

But, in the case of solution, the solid is incorporated with the fluid, and the mixture is always *transparent*. The particles of the solid body become invisible, and provided we do not afterwards make a change in the mixture, by adding something else to it, or abstracting some of its parts, the soluble matter which it contains will never afterwards subside from the fluid, but will remain for ever intimately combined with it.

From these examples, you may form some idea of the innumerable cases of union of bodies, with various degrees of the phenomena I have described and shewn you.

But farther: there are some other particulars remarkable in the greatest number of such cases, which may therefore be

also enumerated among the more general phenomena of mixture. Thus.

4. We find that in most cases we cannot combine such bodies together in every proportion that we might choose. There is a limit to the quantity of the one that can be combined with the other. Thus, water will dissolve only a certain quantity of salt; spirit of wine will dissolve only a certain quantity of camphor; and in a great number of the cases there is even a limit upon both sides to the quantity of each that can be joined with the others and the bodies can be properly united in one proportion only. This limitation to the quantity of a substance which can be joined chemically with a certain quantity of another, is denoted, in chemical language, by the term SATURATION. Substances which are added to one another, or mixed together exactly, in the largest proportion in which they will unite are said to be mutually SATURATED with one another. Of such substances we shall have examples in the salts.

Saturation again is of two kinds, First, simple, as in the case of common salt dissolved in water. Second, reciprocal, where neither of the bodies can be combined with the other except in one proportion, as in the case of a saline crystal.

5. A distinguishing mark of chemical union is, that the substances cannot be separated by filtration, or other mechanical means. It is even difficult, and, in some cases, impossible to separate them by the action of heat.

A volatile substance, when thus united to another which is more fixed, cannot be separated again so easily by heat applied, as we might be led to expect from our knowledge of their difference in point of volatility. When we apply heat to the compound, the more volatile ingredient is not changed into vapour by that degree of heat which we know to be sufficient to produce this effect in its separate state. In some of the cases in which the volatile body is combined with a very fixed one, no heat that we can easily command is strong enough to change it into vapour. In others, though it may be forced to assume the form of vapour, it always requires a heat much stronger than that which is sufficient to produce this change in its separate state; and in many such cases, when the heat is raised so high, the volatile body does not rise

alone; the more fixed body is raised along with it, and the compound therefore is converted into vapour, without being separated into its constituent parts, though the more fixed ingredient would not have been converted into vapour by itself in the same heat. Several of the experiments which have just been exhibited are examples of what we are now saying. Thus, in the case of water and vitriolic acid, we shall be disappointed if we should expect that by heating this mixture to 212°, the water will quit the acid. In the first place, it will not boil till considerably hotter, that is, the volatility of the water is diminished. But farther, the water carries off with it a great part of the acid. Again, in the example of volatile alkali, and vitriolic acid; this mixture will not separate at all by heat. It has nothing of that pungent smell which indicated the volatility of the salt of hartshorn. And, when we increase the heat a good deal, the whole mixture rises undecomposed, and the vapour condenses into a solid salt. Lastly, muriatic acid, and crude calcareous earth: this acid has such a great degree of volatility, that we cannot obtain it in a coherent, or a fluid form; and, when pure, it is always a vapour: Yet, when combined with the calcareous earth, it stands a red heat, long continued, without appearance of separation.

It is, therefore, impossible, in the greatest number of these cases, to separate from one another, by heat alone, the different substances which have been thus combined.

6. But, by multiplying our experiments in the way of mixture, a discovery has been made, which has been of infinite use to us in chemistry, and has greatly enlarged our power over a great number of different compounds. The discovery I mean is, that the addition of some suitable third body to a compound of two ingredients, which are united strongly together by chemical combination, will, in many cases, dispose them to separate from one another. The examples of this are very numerous; but as our purpose at present is only to form a general idea of these facts, I shall only call your attention to a few examples, which will be sufficient to serve this purpose.

Example 1*st*. If we pour oleum tartari (which is the liquor into which pearl ashes melt when long exposed to the air) into a quantity of vitriolic acid, mixed with water, we shall have a violent effervescence, and we shall see a quantity of crystalline salt fall to the bottom. If we now taste the liquor, we shall find its acidity greatly diminished, if not entirely gone. Thus have we procured a separation of the vitriolic acid from the water.

2*d*. The salt which is produced by the mixture of vitriolic acid and the salt of hartshorn, is called vitriolic ammoniac. It has no smell. If this be dissolved in water, and we pour into it an equal quantity of soap-maker's ley, called by the chemists caustic alkali, we shall instantly be struck by the most pungent smell of the salt of hartshorn, greatly exceeding the usual pungency of that salt. At the same time, a quantity of crystalline salt will fall to the bottom, without smell, and having very little taste, in comparison of the vitriolic ammo-niac.

3*d*. If, to the solution of camphor in spirit of wine, which I described a little ago, we add a quantity of water, we shall instantly produce snow-white clouds in the liquor; and, al-lowing the mixture to settle, we shall find all this white matter collect together at the surface, if we have added enough of water, and that it is pure camphor which thus floats at top.

4*th*. If we pour oleum tartari on the solution of marble in the marine acid, we shall observe a copious subsidence of the cloudy matter, which is immediately produced. This falls to the bottom; and, when examined, is found to be the same in all its chemical properties with the marble which we dis-solved. This is an evident separation of it from the acid.

This form of separation, when the detached substance falls to the bottom, is called a PRECIPITATION, and the substance so separated is called the PRECIPITATE.

We have here an opportunity of observing further, in all such cases, that the substance which is added to the com-pound, in order to effect a separation of its constituent parts, unites strongly with one of the ingredients of the compound,

which are thus separated from one another; and we have a new compound.

The four separations just now exhibited are, every one, examples of this. Thus, in the first experiment, the salt at the bottom is found to contain the vitriolic acid united with the salt of the oleum tartari; that is, with pearl ashes. In the second experiment, we find the very same salt at the bottom. In the third experiment, the liquor is spirit of wine, diluted with water, but void of camphor. And in the fourth experiment, the liquor is found to be the solution of digestive salt which consists of marine acid, and pearl ashes.

These examples are a sufficient illustration of the new combination which generally results from the separation by a third body. There is often a gradation in these separations. Thus, silver dissolves in aquafortis, just as marble did in that liquor, but after it is dissolved, it can be separated by mercury. Aquafortis can also dissolve mercury, and the mercury may be separated by copper, lead, or iron. And copper, dissolved in the same manner, can be separated by iron or lead; and lead, so dissolved, may be separated by iron.

7th. There is one other effect of mixture, which is as general as these already enumerated; but the knowledge of it has not yet been found of so much importance to the chemist, and has therefore been less attended to, and is but seldom mentioned in chemical books, though it is a remarkable fact, and worthy of more notice. It is this: that in most cases of the union of bodies with one another, if their nature and manner of union be such as to admit of the proper examination, we find that the bulk of the two when combined, is different from the sum of their bulks in their separate state. It is generally rather less, but in some cases greater. Thus, it is found that 100 cubic inches of water, mixed with 100 of spirit of wine, do not make 200 cubic inches of mixture: they rarely make 198, and often considerably less. Experiments have shewn this also in mixtures of metals with one another, as in the mixture of mercury and silver and still more remarkably, in the mixture of copper and tin. Although tin is the lightest of all the metals, yet this mixture, in a due

proportion, produces a mixture which is considerably heavier than the copper; that is,....a pound weight of tin being mixed with a pound weight of copper, the mixture will be less bulky than two pounds of copper, and far less than two pounds of tin. The same thing happens in mixtures of salts and water. The difference is commonly not great; but it is sensible, and does not proceed from the heat or cold produced. Hence it appears that we cannot form an exact judgement of the pro. portion of the different ingredients in a compound, by exam. ining its specific gravity, though we know what these ingre. dients are, and what their specific gravities are in a separate state. Therefore Archimedes was in an error when he thought that he had solved the problem about the golden crown. I think too that this phenomenon, when attentively considered, will be found to be inconsistent with some com. mon opinions,....as that of the nature of water, and the cause of the difficulty found in compressing it. For it is not enough to say that the other ingredient occupies less room, by lodging in the interstices between the spherules of water, which only touch each other in single points. For, when a certain quantity of some substances are mixed with water, the bulk of the mixture is even less than that of the water alone. *(See Hahn de efficaciâ misturae in mutandis volumini-bus corporum.)* The particles of water must be nearer than they were before; that is, they were not in contact at the first. Mr. Achard of Berlin, and Mr. Brisson of Paris, have made a vast number of experiments on this subject.

But this general fact, of the change of density in conse-quence of the union of bodies into compounds, is not so in-teresting to the chemist as those which I before enumera-ted.

OF THE THEORIES OF CHEMICAL MIX-TURE AND COMBINATION.

In stating the theories of the effects of mixture, I may begin by remarking, that during the first æra of chemistry in Europe, from its first introduction to the time of Lord Verulam, the chemists had no intelligible theory. This was

occasioned, partly by their being too little acquainted with the principles of reasoning that are most familiar to the rest of the observers of nature, and partly by the nature of their science.

When we give an explication of any extraordinary appearance or property of bodies, we always do this by shewing that it is not at bottom so extraordinary, or so very much unconnected with every thing else, but that there is a connection between it and other things, which we know very well, either by a resemblance which it bears to them in certain particulars, or by its deriving its origin from the same causes with more familiar events.

But the chemists, having confined their attention solely to chemistry, were strangers, in a great measure, to the rest of the world. They could not, therefore, explain the chemical facts, by shewing a resemblance between them and other things better known. They were more disposed to explain other things by chemistry, than chemistry by any thing else. Hence arose their attempts to set up a system of medicine, upon chemical principles ; and a number of other attempts of a similar kind. And, in their reasonings concerning the phenomena of chemistry itself, they never went further than explaining one chemical fact by another. Thus, after observing that the salts called acids effervesce with the salts called alkalis ; if they found any new substance not an alkali, which effervesced with acids, they said that it did this because it was of an alkaline nature, or contained hidden alkali in its composition.

But, beside the ignorance of the chemists in other things, it must be allowed that the nature of their science may have been one cause of difficulty to those who would have attempted to improve it in the way of reasoning and explication. Most of the chemical phenomena are so extraordinary, and it is so difficult to find any thing else to which they bear a resemblance, any thing to which they can be compared, and by which they may be explained, that on this account alone, many of the first attempts could not miss to be unsuccessful. It is much more easy to give a satisfactory explication of surprising facts in some other sciences. When we are told

that an ingenious mechanic has contrived to move an immense mass of rock, of enormous size and weight, and this by the force of one man only : if we are at all acquainted with the powers of mechanism, we shall not be greatly surprised at hearing this. We know the thing to be possible, and when we see the machine by means of which it was done, we shall understand his contrivance, and the effect which it produced, completely. We shall perceive that his machine is a system of levers or wheels, and perhaps of screws, or wedges, or pulleys, properly combined and that the whole is a contrivance for increasing force, at the expence of velocity. We shall, therefore, understand how the force of one man might be sufficient to produce the effect; but that he could only move the mass with a very slow, and almost imperceptible motion. But, if the same rock were moved by a chemist, we shall be disappointed if we expect to be as readily satisfied, or to understand as quickly, the nature of his operation. If we are told that he mixed a small quantity of saltpetre, sulphur, and charcoal together,....laid this mixture under the rock, and applied a spark of fire,....and that the rock was instantly thrown up with great violence and velocity out of its place ; we find nothing here that is reducible to the common laws of mechanism....no contrivance for increasing the force at the expence of the velocity :....we wonder.

Of this uncommon and unaccountable nature are the effects which generally present themselves in chemical experiments. At the same time, there is so wide a field of inquiry, and it is so easy for a person who applies himself to it, to make new discoveries of curious and useful facts, that those who formerly employed themselves in improving chemistry, took chiefly this way. They made experiments, and enlarged the collection of facts, finding this much easier than to explain them. Or, if they attempted explications, it was in the manner already mentioned, and which none else understood.

Such was the state of our science until the time of Lord Bacon ; when it became more commonly known to those who were masters of other branches of knowledge. From this period, attempts were made to speak more intelligibly than be-

fore; by endeavoring to explain the chemical phenomena from mechanical principles. I must here observe, that as the mechanical powers of bodies are commonly the most familiar of any, and most distinctly understood, the mind of man is always prone to have recourse to these for explaining the phenomena of nature. We have always a propensity to suppose that any extraordinary effect or power of a body depends on some mechanism in it, something particular in the form, size arrangement, or motion of its parts. And when any remarkable fact or appearance is shewn to depend on principles of this kind, it is generally thought to be well explained. The account thus given of it is plain and intelligible.

Lord Bacon, therefore, recommended this sort of reasoning in preference to the language which was common in his time among the chemists; and Mr. Boyle afterwards attempted to introduce and support it more fully. But the consequence was, that for some time after, this sort of reasoning was applied to chemical phenomena with so much boldness, and so little judgment or consideration, as quite disgusted those who had real discernment; and they formed an opinion, that chemical facts could not be explained in a satisfactory manner by reasoning from mechanical principles.

There is a noted example of a bold reasoner in this way, in a French author, Lemery, who in other respects was a chemist of great merit; but he was led into this manner of reasoning by the fashion of the times. According to this author, a fluid which has the power of dissolving a solid body, abounds with sharp and pointed particles, having the forms of needles or wedges, which are agitated in the fluid with a rapid and confused intestine motion. The solid again has pores of such sizes and shapes, as are fitted to the pointed particles of the fluid, in consequence of which they are penetrated and torn asunder. In like manner, he explained the precipitation by a third body, as when potash causes marble to be precipitated from its solution in aquafortis, by saying that the particles of the potash were porous and spongy, and, by this configuration and confused motion, they took hold of, and broke the spiculæ of the acid, &c. &c. This supposition appeared at first to ex-

plain some particular facts very conveniently. Thus aquafortis dissolves iron or copper, but not gold ; the reason was plain, because iron and copper have wider and more numerous pores than gold. But aqua regia dissolves gold, and does not act on silver. In like manner, mercury penetrates and dissolves gold, and will not touch iron, and very difficultly copper. Many other examples of the same kind might be adduced. Indeed, this sort of theory, in the way in which it was managed, was quite inconsistent with sound mechanical principles, upon which alone, however, it was meant to be founded. The motion of the particles of fluids, necessary for producing the agitation, could not continue ; they must necessarily stop one another. Yet spirit of salt, though kept for ages, will act on marble ; and we have proof by experiment, that it is not necessary that the particles of a body be fluid or moving, to make it capable of acting upon another. There are some examples of solid bodies acting on one another, and mutually dissolving, and we cannot imagine that the particles of solids are moving among themselves.

The experiment which I have often mentioned to you, in which an intense cold was produced by mixing dry salt and dry snow, is of this kind : they melt into a brine, that is, they dissolve one another. In like manner, corrosive sublimate, and regulus of antimony, lime, and crystallized mild volatile alkali, act on each other, and unite in a different manner from their former state, exhibiting a true chemical solution.

It is needless to insist longer on these whimsical theories, so gratuitous and unsupported by evidence, and so deficient even in the plausibility of explanation. We can have no distinct conception of such action, and without this no theory can have any value.

These, and some other attempts of the same kind, to explain the chemical phenomena, appeared therefore quite crude and unsatisfactory to those who had more discernment and penetration than ordinary. But we had no chemical theory that connected this science properly with other parts of our knowledge of nature, until Sir Isaac Newton published that edition of his optics, at the end of which he has given a number of queries, containing his speculations and conjectures

concerning many of the curious and difficult facts which occur in the study of nature.   There are a number of those queries that relate to chemistry.   In these he lays open a view of the more remarkable phenomena which occur in chemical mixtures, which is altogether his own, and which is much more satisfactory, and makes them appear much more conformable to the rest of nature, than any that had ever been offered before.

This great author had already given to the world his much admired system of the universe, in which he explained the connection of the heavenly bodies with one another, and the regulation of their motions, by shewing that they are retained in their orbits by the same power, or principle of motion which determines a stone to fall to the ground ; a power commonly called *gravitation*, or the *attraction of gravitation*.   He conceived that there are other forces or principles of motion in nature, more or less similar to this, by which certain bodies act, or appear to act, at a sensible distance, on one another. This is evident in the case of the magnet and iron, and in electrified bodies.   He suspected that there were still other similar forces, whose sphere of action was still smaller, even so small as to escape our senses.   He imagined that these are manifest in the attraction and cohesion of polished planes, and of metals, in the attraction of small vacuities for different fluids, commonly called *capillary attraction*, and in the inflection and deflection of light, as it passes near the edges of solid bodies.   *It is worthy of particular remark*, that the force which acts in these last instances is amazingly great. Notwithstanding the astonishing velocity of light, about 200, 000 miles in a second, and the almost unmeasurably minute space through which the deflecting force acts on the light, it turns it greatly out of its former direction.   From the quantity of this deflection, we could calculate the proportion of this force to that of gravity : for Newton, with wonderful sagacity, has been able to give us the exact measure of the distance at which this deflection is performed.   This force is not less

than twenty millions of millions of times greater than gravity*.

Sir Isaac, reflecting on these examples of the action of bodies on one another, and being struck with the effects he had observed in making chemical mixtures, suspected that these last might depend on powers, or principles of motion, somewhat resembling the others.

You cannot have a more striking view of his ideas on these subjects, by any account of them, than that which you will get by reading those passages at the end of his Optics, (Quest. 22, &c.) in which he first communicated these conjectures of his to the world. His style is so simple, and at the same time so nervous, that it cannot fail to make an impression.

By these Queries, in which Sir Isaac Newton first proposed the doctrine of attraction in chemistry, an intelligent chemist cannot miss to perceive that he had acquired extensive and accurate knowledge in the different parts of this science : and, when we consider the time when they were published, and how little had been done to explain the chemical phenomena in a satisfactory manner, we see in these queries a proof of that capacity of mind, and that acuteness and originality of genius, by which Sir Isaac distinguished himself on every occasion. Not only is the path pointed out, in which great reputation has been acquired by some of the most eminent chemists, but the chief steps are actually made, and the manner in which farther progress may be made, is distinctly described.

His explanation of the chemical phenomena was received, by the unprejudiced, with the highest admiration and pleasure : and there is no doubt but that it opened a view of some principles of action in nature, which certainly exist, and which, with some few modifications, explain many of the most remarkable chemical facts. It is evident that there are such *principles* of motion in nature, as Sir Isaac pointed out; for

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* Dr. Herschell's discovery of the refrangibility of heat, in precisely the same manner and degree as light, shews the immense force which connects it with other matter; a force fully adequate to the most remarkable effects which we see it produce.

there is no denying that there are such *motions*. The reality of the powers which connect the particles of matter is clearly proved by a multitude of facts and experiments: and, that attractions somewhat resembling these, act in chemical mixtures, and that with great force, is equally plain from the phenomena.

For, when marble, for example, or iron, are dissolved in the muriatic acid with rapidity, and appearances of violence, a very great force must act to produce this effect, overcoming the strong cohesion of the particles of those bodies. Were we to attempt to divide them mechanically into very minute parts, it would cost us as much labour and toil to effect this ; and our utmost exertions, though ever so long continued, would never divide them into atoms so minute as those into which they are divided by chemical solvents. And, even after they are divided by these, we have the plainest evidence of the continued action of an attractive force, which prevents the heavy matter of the solid from being ever deposited again by the less ponderous fluid, and which binds to the particles of the iron or marble, the particles of the volatile solvent, thereby greatly diminishing, or repressing, the volatility which it had in its separate state.

Sir Isaac's theory, therefore, explains all the most difficult parts of the subject. It supplies a force by which solid bodies are divided, and remain for ever suspended. It explains why some salts are deliquescent, and why volatile and fixed substances, when united in this way, are afterwards so strongly connected together, and so difficulty separated from one another by heat.

I must not, however, omit to observe, that some of the effects which were at first imputed to the violent action of these forces, have been since discovered to proceed from other causes. Such is the effervescence which occurs in making many chemical mixtures. Sir Isaac supposed it to proceed from the violent friction and collission of the particles of the different bodies, while they rushed together to unite. It was very natural for him to form this supposition. It was even, I may say, a beautiful part of his

theory, as it presented to the imagination, an explanation
which appeared at that time very probable, and was easily
understood. (See note 15, at the end of the volume.) It
is known however now, by late experiments and discoveries
to proceed, in most cases, from the liberation of some
elastic aerial matter, which was contained in those bodies
in a condensed state. And the heat is now explained on
other principles, viz. by a diminution of capacity in the
compound, to contain the heat which was contained in its
ingredients, or (to express it more cautiously) by the libe-
ration of a quantity of heat not necessary for that tempera-
ture of the compound. But all the other phenomena of
mixture are still considered as depending entirely on these
specific attractions.

Notwithstanding the very great merit of this theory in
comparison with others, it was not very well received at
first by the chemists abroad. They objected to the word
attraction, as implying either an active quality in matter,
which we cannot conceive to be possessed of activity, or
some connecting intermediate substance, by which the par-
ticles of bodies were drawn together, and for the existence
of which no proof is offered. They therefore chose to
substitute the term AFFINITY instead of ATTRACTION.

But their objections to the word *attraction* were certainly
unreasonable. Sir, Isaac in the beginning of these que-
ries, expressly declares, that when he uses this word, he
does not pretend to assign the causes, natures, or manners
of acting, of those forces by which bodies are disposed to
rush together into union. He only means the forces them-
selves, or the disposition to join, manifested by the fact
itself, leaving it to others to discover the cause, and ob-
serving that it is improper to attempt it, until we become
better acquainted with its manner of acting, by studying
the facts. And when the word is used with this precau-
tion, merely as a term expressing a fact, a phenomenon,
I do not see any advantage in preferring *affinity* to it. It
would sound very ill, to speak of the affinity of gravitation,
of electricity, or of magnetism. Attraction is more ex-
pressive; and affinity implies, or suggests, some similarity,

which, in most cases, is not agreeable to fact, seeing that we generally observe the greatest dissimilarity in those bodies which are eminently prone to unite. (See note 16, at the end of the volume.)

As Sir Isaac's theory, however is now well established wherever the science of chemistry has made any progress, and, as we shall find it most extensively useful to us in assisting us to understand a great variety of the chemical phenomena, I shall consider it with more attention, and endeavor to give you as clear and correct an idea of it as possible, as well as of the facts which it explains. With this intention, I shall make a few remarks on it.

1. The first I shall offer is, that it differs from some other kinds of attraction in this respect, that, so far as we can perceive, it cannot set in motion large and visible masses of matter. It only acts in such a manner, as to dispose the minute and invisible particles of one substance, to separate from one another, and unite themselves severally to the minute and invisible particles of another substance. An obvious example of which we have in a lump of salt or sugar, and a quantity of water.

It is a necessary consequence, therefore, of the *nature* of these attractions, that the attraction of *cohesion* is an antagonist to them, or counteracts them more or less in all cases. By the *attraction of cohesion*, or the *cohesive attraction*, is meant that attraction which similar particles, or the particles of the same kind of matter, manifest for one another. (See note 17, at the end of the volume.)

We have an obvious example of the action of this sort of attraction in two bits of lead, or of elastic gum, which being made quite clean, and closely applied to one another, will join and cohere as strongly as if they had been melted or soldered together, in those parts of their surface that have been brought into very close contact. Iron affords another example ; different pieces of it uniting perfectly together, when they are softened by a strong heat, and very closely applied. Clay is an example still more familiar, and, in my opinion, perfectly similar to all the other examples. Although, in these cases, we do not

perceive the action of cohesive attraction, in making the masses cohere, except when they are brought into what appears to our senses the closest contact ; yet other experiments have shewn that it can act at small distances, only that its power is diminished so fast as the distance is increased, that it soon becomes imperceptible. The experiment that I have chiefly in view here, is that first made by Mr. Huyghens, and mentioned in the Phil. Trans. No. 86. He found that two finely polished plates of glass attracted each other, and that the one lifted the other, even when a single fibre of silk was interposed, but not when two fibres crossed one another.

Without this cohesive attraction, no body would have solidity or hardness, but would be a powder, or a liquid, or in the form of vapour, if heat were present, or in the state of inconceivably subtile unconnected particles. This attraction is in general strongest in the most solid, hard, and dense bodies ; less strong in those which are soft or brittle ; particularly modified in those which are tough and ductile. Its effects are reduced to a great degree of weakness in those which are fluid, and almost to nothing in those which are in the form of vapour*. It is in all these cases counteracted, less or more, by the repulsive force of the matter of heat.

This sort of attraction (viz: cohesive), as you will perceive, does not in the least counteract some other kinds of attraction, by which certain bodies act on one another, such as the attraction of the magnet and iron, and the attraction of electricity and of gravitation. But the chemical attractions are directly counteracted and opposed by it. When a lump of iron, for example, is thrown into nitrous or muriatic acid, there is a powerful chemical attraction, which disposes the particles of the iron, and those of the acid, to unite together. But it is evident that the cohesion of the

* The immediate, or what we may call the *formal* cause of these different modes of aggregation, are distinctly, but concisely, explained in Newton's questions, at the end of his optics. They are considered at great length by Mr. Boscovich, in his Theory of Natural Philosophy, a work of the greatest ingenuity, and richly deserving the *serious* perusal of a philosophical chemist.
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parts of the iron with one another is in opposition to their union with those of the acid; and were not their attraction for the acid particles stronger than their cohesion with one another, the iron could not be dissolved. It is stronger, however and the consequence is the dissolution of the iron, notwithstanding the strong cohesion of its particles. (See Note 18, at the end of the Volume.) But although this cohesion is not strong enough to preserve the iron from being dissolved, it must necessarily resist or counteract, to a certain degree, the powerful attraction of the acid : and the iron would be dissolved with much greater rapidity, were it in our power to deprive it of its cohesive attraction before it is put into the acid.

There are even examples, in which, by increasing the power and action of cohesive attraction, we can enable it to overcome a chemical attraction which it was not able to overcome before. In the hammering of iron, and other metals, the latent heat is forced out, the chemical attraction between it and the metal being overcome, by increasing the power of the cohesive attraction, in consequence of the condensation of the parts of the metal, which the hammering produces*. The metal, after being hammered, is both denser and harder than before. And the cohesive attraction, being in all cases an antagonist to the chemical attraction, by which latent heat is retained in bodies, when the one of these attractions is increased the other must give way or be overcome more or less. Even the chemical attraction of acids for iron is counteracted, and reduced in its power and efficacy, by hardening the iron.

In confirmation of these remarks, I observe that we have learned, by the most extensive experience in chemistry, that bodies which have chemical attraction for one another, act and unite together much more readily, when we take them in a state in which their cohesion is weak, than when it is strong. Of this you will see numerous examples as we proceed. We have a good example of this counteraction of cohesive and chemical attraction in the phosphorates of silver and of nico-

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* See what was delivered on this subject in page 140. The diminution of the chemical attraction, by hardening the iron, is very doubtful. EDITOR.

lum. These compounds retain the phosphorus while they are fluid by heat, but when the cohesive attraction increases by the diminution of the heat, the phosphorus is thrown out, and burns on the surface of the metals, while they congeal *(Annales de Chymie,* T. 13.*)*

And this proceeds from that peculiarity which I mentioned just now in the nature of chemical attractions, that they do not act so as to dispose large, or any way perceptible masses of matter, to move towards one another, and join together in their massy state. They only act so as to dispose the minute and invisible atoms of one species of matter to unite severally with the minute and invisible atoms of another, or of some others. And these attractive forces between the atoms of such different kinds of matter, act so powerfully as to tear asunder and totally destroy the masses which these different kinds of matter formed separately before.

2. We may in the next place remark, with regard to this attraction, that when two bodies unite in this manner, they are each of them divided into particles of such extreme minuteness, that the utmost efforts of imagination cannot form an idea of it. I had one day a desire to see into how many sensible parts a body may be actually divided by solution. I found that one grain of silver gave a very sensible tinge to about 38 troy ounces of water, or 18060 grains. I found by repeated trials, that a drop of water weighs very nearly a grain, for 50 drops weighed from 49 to 55 grains. An ordinary microscope exhibits 100,000 sensible parts in a drop when spread out. Suppose that it exhibits only 1000 such parts: in every one of these there are many particles of the silver, for the visible particles are all little flocculi, of different ragged forms, and therefore consisting of many atoms. From this observation, I was well entitled to say that a grain of silver could be divided into much more than 18 million of sensible parts. You may form some notion of this number by this circumstance, that this number of common pease, laid close to each other, would occupy 81,43 miles. This is not a singular or extraordinary example; we have reason to think that a similar division takes place in all cases.

3. I observe that the influence or energy of chemical at-
traction reaches only to an exceedingly small distance round
the particles of bodies, a distance so extremely small, that it
is not perceptible; and, if we trust our senses, no action
seems to take place until the particles come into the closest
contact.

By attending to these particulars in the nature of chemical
attraction, we are enabled to understand the propriety of some
canons or rules relating to the practice of chemistry, or to the
most proper manner of performing many chemical operations,
or of making experiments.

First rule. When it is desired to make two bodies act che-
mically on one another, to produce any change, or to obtain
any product in chemistry, it is in most cases necessary that
one or both be fluid when they are mixed, or that they be ren-
dered fluid, or disposed to fluidity, immediately after. The
cases are very few indeed, where any considerable action can
be induced without previous fluidity. You will get a very
clear conviction of its utility, by taking a small quantity of sal
ammoniac, and of salt of tartar in dry powder, and mixing
them. No sensible change is produced; but pour in a little
water, and instantly you have a strong smell of spirits of
hartshorn. This is instantly formed by the assistance of the
water.

In like manner, dry acid of tartar, and dry salt of tartar, re-
main inactive when mixed; but a little water being poured in,
you have the violent action of the ingredients, and obtain a
compound salt.

The dry salt composed of copper and aquafortis, being
wrapped up in tinfoil, also remains inactive; but if, before
wrapping up the salt, you very slightly wet the tinfoil with a
pencil dipped in water, and quickly roll it up after sprinkling
the salt on it, you have a most violent action, accompanied
with explosions, and even a bright flame and burning heat.
The tin leaf is all corroded or eaten into holes.

The propriety of this rule arises from many considerations.
In the first place, the fluidity produces an extent of contact
unattainable in any other way. The finest powders touch

each other but in few points, and the greatest part of each particle of the powder lies from another far beyond the reach of chemical attraction. But the very atoms, when floating in the fluid, are applied to the other substance all round, at a distance fully within the sphere of action.

But, secondly, it appears that some chemical actions evidently assist one another, not perhaps directly, but by counteracting the cohesive attraction in the particles of solid matter. The cohesive attraction is unquestionably an antagonist to all chemical actions whatever ; it *must* be overcome in the first instance. This is one of those chemical facts or laws, which the chemists on the continent call examples of *predisposing* affinity. Some curious examples will occur afterwards. Perhaps we may not understand the cause or nature of this fitness or predisposition in every case ; but we may use the term as the expression of a phenomenon, an undoubted and important fact,....important, as it very often enables us to accomplish what would otherwise be attempted in vain.

For similar reasons, it is not unfrequently the practice to convert substances into vapour, in order to make them unite. Here the cohesive attraction is completely destroyed. The vapours are most intimately mixed, and they unite when condensed. There are several instances, however, where two gases, that is, permanently elastic fluids, shew no disposition to unite when completely formed, yet will unite, if one of them be presented to the other in its nascent state, that is, in the very act of formation. This happens in the case of inflammable and vital airs.

Second rule. That the solution of solids in fluids is promoted by previously dividing the solid mechanically, or otherwise enlarging its surface, as by pulverisation, granulation, filing, plating. The reasons for this are too plain to require illustration. These preparations increase the surface of action.

Third rule. Frequent agitation of the solvent also promotes solution, by removing the saturated parts of it. This is a curious subject, and of most extensive influence. *(See note* 19, *at the end of the volume.)*

A Fourth Rule is, That the action of bodies in general upon one another, and their union together, is promoted by moderate heat. This rule is founded upon a very extensive experience. I do not remember any exception from it, any example of bodies disposed to act on one another, in which the action is not promoted and increased by a moderate increase of their heat. Thus salts and water act in general more powerfully as the mixture is hotter. There are even examples by which we might be led to conclude that chemical activity would not exist without more or less heat. When we diminish heat, the chemical action of bodies becomes languid ; and in some few cases, when it is greatly diminished, chemical action ceases entirely. This is the reason why we cannot produce a greater cold with salt and snow ; and it is plain that the power of nitrous acid and snow is limited in the same manner, as in a very intense cold the acid shews a diminution of its attraction for the water by crystallizing. Query, Is this occasioned by the increased action of cohesive attraction, in consequence of the diminution of heat, which is an antagonist to it ? Does a gentle heat promote chemical action and union by weakening the cohesion of the aggregate, while a strong heat weakens, in many cases, even chemical union, by introducing a repellency of the parts of the compound, of different volatility ?*

As the general rule, that the chemical action of bodies, and their union with one another, is promoted by moderate heat, has been long established upon the most extensive experience, it has given occasion to some operations, which are often performed to promote the chemical action of bodies upon one another, such as, DIGESTION, CIRCULATION, and COHOBATION.

* The experiments of Mr. Berthollet, and of Mr. Seguin, shew some very remarkable changes, and alternations in the affinities of bodies, by a gradual increase of their heat. They may be explained by supposing that the attractions of different bodies vary in different proportions, by the same variation of temperature ; so that in one temperature they are in equilibrio, and inactive, but not in others.

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OF ELECTIVE ATTRACTIONS.

The remarks which have been now made are equally appli-
cable to all cases of chemical action, and the union of bodies
with one another. But we must now give a little more atten-
tion to those numerous cases, in which a third body frequently
acts on a compound of two ingredients, so as to separate these
from one another, and join itself to one of the two. An at-
tention to these cases is of the greatest importance in chemis-
try; for it is by our knowledge of them that we are enabled to
obtain the decomposition of many compounds, the ingredients
of which cohere too strongly to be separated by heat, or by any
other means. Indeed, I may almost say, that a complete
knowledge of the general laws of this phenomenon is nearly
the whole of chemical science.

The cases in which this method may be employed are very
numerous. There are numerous examples in chemistry, in
which the same body has a chemical attraction for each of a
number of others, or a disposition to unite with them. But
such is the nature of that power by which these unions are pro-
duced, that the body is capable of being firmly joined to one
only of these others at the same time. Whenever two bodies
are joined by chemical attraction, a third can seldom, if ever,
be admitted into the compound. The disposition of these two
to unite with others, is diminished, and, so far as we can per-
ceive, it is diminished the more, in proportion to the force
with which these two are joined together. If this force be
strong, the attraction of other substances is rendered ineffect-
ual; or, if it have any effect, which I think it has in many
cases in which this is not suspected, it is only to weaken a
little the force with which the two bodies are joined, but not
so much as to separate them. But if, on the other hand, the
force with which two such substances are united, is weak, and
we add a third, which has a strong attraction for one of the
two, it immediately unites itself to that one, and separates the
other, or, the other is let go, and separates. Thus we form a
new compound; and this new compound can, in many cases,
be decomposed in its turn, by another body, in the same

manner. Examples of this are more especially called ELECTIVE ATTRACTIONS, or EXCHANGES. (See note 20, at the end of the volume.)

Sir Isaac Newton has not stated expressly any other supposition to explain this phenomenon, than that the added matter has a stronger attraction for one of the ingredients of the compound, than these have for one another. And no doubt this is one necessary cause ; and accordingly, we generally find it more difficult to decompose by heat, the new compound thus produced, than the former one, supposing the volatility of the volatile ingredient, and the fixedness of the fixed one, to be the same in both cases. But still this supposition alone, of a stronger attraction, seems hardly sufficient; for why should not the more strongly attracted body unite itself to that ingredient which attracts it, without dislodging the other, and thus produce a compound of three ingredients, two of which, though without attraction for one another, might, however, be combined by their attraction for the third ? As this happens but very rarely, there is reason to suspect that there is a cause of the decomposition of the first compound, and this is possibly a repulsion between the added body and that ingredient which it separates and throws loose, while it unites with the other. There are many phenomena in chemistry that appear to suggest such a supposition and Sir Isaac himself throws out a hint to this purpose, in another part of his queries. But we shall have better opportunities hereafter, to point out some cases in which such repulsions appear to take place. (See note 21, at the end of the volume.)

It was said just now, that the knowledge of the different cases of elective exchanges enables the chemist to resolve into their principles a great number of compounds, which it would be impossible to decompose by any other method, and that they are, therefore, the foundations of a great number of curious and useful operations.

The celebrated Mr. Geoffroy of the French academy, perceiving what advantage it would give in chemistry to have a distinct and comprehensive view of all the cases in which such exchanges may happen, first thought of reduc-

ing them into the form of a table, which presents them to view at once, and he called it the TABLE OF ELECTIVE ATTRACTIONS, or of *exchanges*, or of *affinities*. It is not proper at present to shew and explain this table, because you are not acquainted with the facts to which it refers. But to give you an idea of the manner in which it is constructed, observe, that it has been found by experience, that a number of different metals can be dissolved in aquafortis, in the same manner as salt is in water. It has likewise been discovered that these, by their different degrees of attraction for aquafortis, are capable of separating one another, in a certain determinate order, by elective attraction. Mr. Geoffroy, in order to recal these facts to the mind, constructed one of the columns of his table in this manner: he placed at the head of it nitric acid, or aquafortis, and under it the different metals which may be dissolved in that acid, placing iron uppermost, next under it copper, under copper lead, under lead quicksilver; and under that silver: thus,

> Nitric acid
> Iron
> Copper
> Lead
> Quicksilver
> Silver.

This arrangment is equivalent to saying at full length, that if, to a solution of silver in aquafortis, we add a little mercury, the silver will be separated from the acid, and we shall have a solution or compound of aquafortis and mercury; if we add to this solution a bit of lead, the mercury will fall to the bottom, and the liquor will be a solution of lead in aquafortis. In like manner, copper will separate the lead, and iron will separate the copper. Also, iron will separate lead, or mercury, or silver; and lead will separate silver. In short, any of the inferior metals will be separated by any of those placed above it. Such a column represents a train of chemical operations.

This was a very ingenious thought of Mr. Geoffroy, and his table was received with general applause, and has been exceedingly useful to the chemists. It was construct-

ed with that skill and accuracy, which might have been expected from his extensive knowledge and great experience in chemistry. But, being the first attempt of this kind, it could not be supposed to be entirely perfect; and, accordingly, succeeding chemists have made a number of objections to the construction of different parts of it. With respect to those objections, I shall only remark at present, that these attractions are capable of being affected by some circumstances, which, if they had been attended to, would have given chemists more distinct ideas on the subject, and might have prevented some of those disputes. One example of this is in the power of heat, which, when varied in its strength or intensity, produces a considerable difference in the force wherewith certain bodies attract one another, or remain combined. This happens remarkably, when one of these bodies is fixed, and the other volatile. If both have the same degree of volatility, increase of heat will not produce (so far as we know) any variation of their attraction, unless it be perhaps to increase it. But when the one is volatile, and the other fixed, variation of heat produces a considerable difference in the force of attraction. There are degrees or intensities of heat, which will weaken the attraction and connection of two such bodies, and, in some cases, will actually make them separate from one another; and, therefore, must make them very liable to be separated by the attraction of other bodies, which would not otherwise have that power. This is a fact, of which you shall be fully satisfied hereafter, when we enter into a particular account of the objects of chemistry*.

There is another circumstance which occasions some variations, or apparent variations, of the elective attrac-

* There are other cases, less remarkable, but equally constant, where it appears that the effect of heat in weakening the attractions of bodies, does not vary in the same degree, in different substances, which do not sensibly differ in volatility. And there are a few cases, where intermediate compositions obtain, in complex subjects, in certain temperatures, by which the mutual actions of the ingredients are often changed. This frequently happens in watery solutions.

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tions; variations, which, when they were first discovered, were so unexpected, that they appeared to overthrow the whole system. The circumstance I mean is the *quantity* of an attracting substance, that is applied to another, or to a compound*. When the quantity of such attracting substance is great, its power over the other, or the amount and effect of its attraction, is thereby increased; the large quantity of the substance acting at the same time, having thereby the more power. This, in some cases, enables a more weakly attracting substance to overcome the attraction of a stronger, when the more weakly attracting substance is used in much greater quantity than the stronger. There are many examples of this in chemistry. And it appears to be for this reason that an abundance of the matter of heat, or caloric, overcomes more or less the cohesive attraction of bodies, and either expands them, or unites itself with them in the form of latent heat, and then melts them or converts them into vapour. (See note 22, at the end of the volume.)

Upon the whole, Mr. Geoffroy's table, considered as the first thought, and first attempt in this way, is highly worthy of praise. But such a multitude of discoveries in chemistry have been made since his time, that his original table gives now but a narrow and imperfect view of the numerous facts which a table of this kind can be made to represent and suggest. Accordingly, several improved and more comprehensive tables of elective attractions and exchanges have been drawn up by different chemists; but none has employed so much pains on this subject, or given such a comprehensive and copious table, as the late Professor Bergmann of Sweden, whose works on this and other parts of chemistry are translated into our language. And since that time, Mr. Morveau in the New Encyclopædia. (See note 23, at the end of the volume.)

* See Decompos. par l'Acide Marin de plusieurs sels vitrioliques er nitreux, par M Cornette. Mem. de l'Acad. 1779.

See also the later publications of Mr. Berthollet, on affinities, in which he has considered this circumstance with great discernment.

<div align="right">EDITOR.</div>

Among the most remarkable additions which have been made to these tables, since Mr. Geoffroy's first attempt, are the cases of DOUBLE ELECTIVE ATTRACTIONS. These had been but little attended to, or understood in his time, although they happen in many important operations in chemistry, and the knowledge of them is indispensably necessary on many occasions.

By a double elective attraction, or double exchange, is meant, the action of two compounds on one another, each of them consisting of two ingredients, and each of those ingredients having an attraction for one of the ingredients of the other compound. The consequence of applying two such compounds to one another is, in many cases that they are both decompounded, and two new compounds produced in their stead; each of the ingredients of the one joining itself to that ingredient of the other compoundfor which it has an attraction.

As the knowledge of these cases is very useful, I began in the year 1756 or 1757, to represent them by diagrams, and to subjoin these to the table of elective attractions, which I gave to the gentlemen who attended my lectures; and some other chemists began soon after to do the same thing, with perhaps a different mode of expression. When I explain the table, I shall shew you some of the ways in which they may be represented. I cannot just now enter more particular into this subject, as the different sub- stances which have attractions for one another are not yet known to you.

Before I conclude this part of our course, it will be pro- per to explain to you some terms of distinction which will frequently occur in the course of your reading, especially of the foreign writers.

The union of *aggregation* is that where a number of par- ticles of the same kind are retained in one mass, called the *aggregate*, while the particles of which it consists are called its *integrant* parts, seeing that their sum constitutes the aggregate, the integer, or whole. These terms express very well the mere relation of co-existence, but I think that the term attraction of aggregation ill expresses the bond of union. It does not characterise it so well as the

common term of *cohesion*, which I should therefore prefer. It is thought proper, however, to distinguish this bond of union of particles of one kind from that which essentially connects particles of different kinds, and is more peculiarly chemical. The attraction of aggregation or cohesion may be overcome by mere mechanical force, whereas the other requires elective or chemical attractions.

The attraction of *composition* expresses very precisely and perspicuously the force which unites particles of different kinds, composing of them, so united, particles of a kind different from both. The attraction of composition unites a particle of vitriolic acid with a particle of fossil alkali, and these two united compose a particle of Glauber's salt. Each of the component particles is called an *ingredient*, and the united particles form a particle of the *compound*. Particles of this kind, or ingredients, cannot be separated by mechanical force, but require the action of heat, or of a third kind of ingredient, to effect a separation.

Of this kind of union there are different classes, which chemists have attempted to distinguish and mark by different names.

When different metals are mixed together, we obtain particles which retain the former properties of the ingredients. In like manner, the mixture of different liquors is frequently attended by no change of chemical properties. The same thing obtains in the solution of salts in water, of resins in alcohol, of sulphur in oil, &c. Yet is the combination of chemical, and cannot be undone by mechanical forces. Such combinations are usually called *mixtures*.

In other cases, the properties or sensible appearance and chemical action of the ingredients are totally changed, and new ones induced. Thus, aquafortis, and soap ley, are two of the most acrid and corrosive substances in nature ; their taste is intolerable, and they quickly destroy our organs ; but, united, they compose saltpetre, a mild and harmless salt.

This is true chemical *combination*, or *composition ;* and the substance produced is a true chemical compound. And

this combination is also distinguishable by mutual satura-
tion of the ingredients, whereas a mixture is possible
with any proportion of them.

These two kinds of combination are not so perfectly
distinct that no exceptions are to be found in some cir-
cumstances.   Thus salt and water, in a certain determin-
ate proportion, form a saline crystal.   Here is saturation,
but the chemical properties are scarcely changed.   Tin and
copper mix in any proportion, but the mixture has all its
sensible qualities exceedingly different from those of the
ingredients.   It is highly elastic, brittle, sonorous, bril-
liant, and dense; whereas the ingredients were ductile,
soft, unelastic, and comparatively light.

Still, however, the distinction is useful when attended
to.

The older chemists gave the name mixt to chemical
compounds consisting of two ingredients, which we have
never been able to reduce to simpler ingredients.   Parti-
cles of a mixt, compounded with particles of another mixt,
formed particles of a *compound;* the union of two com-
pounds formed a *decompound:* and the union of two de-
compounds formed a superdecompound, &c. &c.

Modern chemists no longer use those terms, but say,
compounds of the first order, of the second order, and so
on.

GENERAL OBSERVATIONS.

This doctrine of chemical affinity is, unquestionably,
the great and distinguishing principle of the science, as
the laws of motion are of mechanical philosophy.   The
great advances made in the latter science have derived
but little assistance from the speculations of ingenious
men about the foundation of the *primary* laws of motion,
and the intimate nature of moving forces.   Indeed it has
been greatly obstructed by these speculations, and by the
attempts to explain all forces by impulsion.   Kepler and
Newton acquired their immortal fame by the discovery
and exact description of the *secondary* laws, the great or
general facts, really observed in the operations of nature;

which result, indeed, from the primary laws of motion, and are the modification of those laws, by the circumstances of the particular cases. Such are the three laws of Kepler, and the great law of gravitation, which includes all the three.

I apprehend that chemical science is, in like manner, obstructed by speculations about the principle of affinity, and, particularly, by the attempts of ingenious men to explain the chemical operations by attractions and repulsions. Even in mechanical philosophy, attraction is a metaphorical term; and it is a mere figure of speech to say, that a loadstone *attracts* iron. But the resemblance in effect to real attraction is very great; and I mean nothing by the term but this resemblance. I mean only that the thing attracted actually approaches the attracter. But, in chemistry, even this is not observed in all the operations which we explain by attraction. In them all, the supposition is gratuitous, and, in many cases it is false; for often the density of the compound is less than the medium density of the ingredients. Therefore there is not that *local motion* and *approach* which alone authorise us to consider the event as the effect of an attractive force. In chemistry, the metaphor, both in the expression and the thought, is double. Union of things, formerly unconnected, resembles, in a certain degree, the bringing into contact things formerly removed from each other: and this approximation resembles the effect of a real attraction, an actual pulling the thing nearer to us with the hand.

I may venture to say that no man ever got a clear and really explicatory notion of a chemical combination by the help of attractions. When we have, in imagination, endowed a piece of copper wire with an attractive force, regulated by any law we please, and think that this will explain how it acts on a solution of nitrate of silver, or upon water, the mere communication of the piece of copper with the rest of a galvanic circle changes the whole operation, and it either oxydates, or deoxydates, according to the end of the pile with which it communicates; and now, the particles which are supposed to exert an attractive force, or to attract, give evidence of repulsion,

and *vice versâ* although no change of situation or distance been made in the particles attracted or repelled.

The chemist will do more wisely, if he forego all specula-tion about the ultimate internal action, and direct his whole attention to the external phenomena. Let him notice every general fact in the phenomenon, and class them according to their extent, and carefully note the substances which are the subjects of these general phenomena; these phenomena give him all that he can discover of the nature of those substances. If he attempt to explain the internal motion of the atoms, which are the only true indications of the moving forces, he will find it impossible. He may fancy a certain attractive force and then deduce certain motions from its action. But all the mathematicians in Europe are not qualified to explain a single combination by these means. When he speaks of attraction and affinity, let him affix no further meaning to the terms than a certain faculty of combining; and when he says that the attraction of aquafortis for potash is greater than for silver, let him mean nothing more than this, that aquafortis combined with silver, will be separated by potash, but silver will not separate aquafortis from potash. We are altogether ignorant of the comparative intensities of the mov-ing forces, and what forces co-operate, and what counteract each other in chemical operations. These depend as much on the circumstances and manner of action, as on the intensity of the forces. The only explanation to be received of a chemical phenomenon is, to shew that it is a case of a more general phenomenon already known. It is thus only that even a mechanical phenomenon is explained. The lunar irregularities are explained by shewing that they are cases of universal gravitation. The truly chemical explanation is ac-complished by shewing that the circumstances of the pheno-menon are precisely such as result from the acknowledged laws of that phenomenon from which we deduce the explana-tion.

Let chemical affinity be received as a first principle, which we cannot explain; any more than Newton could explain gravitation, and let us defer accounting for the laws of affinity,

till we shall have established such a body of doctrine as he has established concerning the laws of gravitation.

We may sometimes have it in our power to assist the mind in conceiving a chemical phenomenon, by comparing it with a mechanical phenomenon. Thus, the chemical phenomena arising from galvanism may be conceived a little better, by comparing them with some magnetical phenomena. The crystallization of salts may be illustrated by comparing the particles to little magnets. But this is not explanation; nor is it even an illustration, unless on the supposition that we understand something of the mechanical phenomena of magnetism.

The late dissertations of Mr. Berthollet, on certain chemical affinities, are full of information, *purely chemical*, and are more free from ill-judged applications of mechanism, than most other chemical writings. Yet they are not entirely free, and occasion will be taken to mention some cases in which his explanations are incompatible with the mechanical principles which he certainly adopts. Mr. Seguin, also, has made some very judicious observations, in his Dissertation on the changes of chemical Affinity by heat, published in different volumes of the *Annales de Chymie*.   EDITOR.

PART III.

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CHEMICAL APPARATUS.

AGREEABLY to the plan which I laid out in beginning this course, we have finished the first and second division of the general doctrines of chemistry, and shall now begin the third. Under this I proposed to give an account of the nature and uses of the apparatus or instruments usually employed in performing the chemical operations and experi-

ments. This is a subject which you are very well prepared
to understand, by what has been already premised, in a gene-
ral way, upon the subjects of heat and mixture. And it is
necessary to take a view of it before we begin to consider the
objects of chemistry in a more particular manner.

But under the title of the apparatus, or instruments, I do
not propose to take under consideration the nature and proper-
ties of water, of air and of earth, as Dr. Boerhaave has done,
who has given elaborate treatises upon these four subjects,
under the title of instruments of chemistry. It is true that
we apply heat to produce changes and effects upon different
bodies, and that when we employ it in this way, it may be
called, in metaphorical language, an instrument in our hands.
It is also true that we make use of water on many occasions,
as an assistant by which we can reduce some bodies into a
state of fluidity, and mix them more effectually together. But
if, upon this account, we must treat of fire and of water as
instruments, for the same reasons, and with the same pro-
priety, I might bring most of the objects of chemistry under
the same denomination : for, after we have learned by expe-
riments the powers or qualities which belong to those bodies
we take advantage of this knowledge, and employ them ac-
cordingly to serve our purpose on different occasions. Thus,
for gilding, mercury is an instrument; for separating some
metals from their ores, iron, and for others lead ; various salts
for separating metals, and other bodies ; black flux for reduc-
tion. But to treat of iron, and lead, and quicksilver, under
the title of instruments of chemistry, would be a very improper
and preposterous method. Dr. Boerhaave has been led into
this method by the metaphorical language of some of the old
systems of chemistry, in which it appears that earth, for what
reason I cannot tell, has been likewise considered as an in-
strument and is accordingly treated of as such by the Doctor.

For my part, under the title of instruments, (I mean what
is meant in common language) utensils or furniture neces-
sary for performing the chemical operations and experiments.

When we reflect upon the different particulars of which a
chemical apparatus consists, we find it may be conveniently

divided into three parts. 1. Vessels for containing the sub-
jects upon which the effects of heat or mixture, or both to-
gether, are to be produced. 2. Means of producing heat.
3. Means for applying it in the best manner to the subjects
on which it is to act, and for regulating its force.

We shall consider the chemical apparatus according to this
arrangement, and first therefore describe the vessels.

The variety which obtains among chemical vessels, in con-
sequence of which they are fitted for different purposes, de-
pends partly upon the *materials* of which they are made and
partly upon their *form*. I shall first mention the variety of
materials which we find it necessary to employ in the con-
struction of chemical vessels, and afterwards describe their
differences in point of form.

The remark with which Mr. Macquer sets out in treating
this subject is very just; that were it in our power to have
vessels completely adapted to chemical purposes, we should
wish them made of materials possessed of these qualities.

1*st.* We should wish them transparent, that we might see
distinctly what goes on in them.

2*dly.* Qualified to resist the action of the most corrosive
substances.

3*dly* To bear sudden alterations of heat without breaking

4*thly.* Strong, to confine elastic vapours.

5*thly.* To bear violent heat without melting or being other-
wise injured.

But, as there is no matter in nature possessed of all these
qualities, chemists are under the necessity of constructing
their vessels of different materials, which have some of those
desirable qualities, though they want the rest; and in each
operation or process, they use vessels made of materials which
have the qualities more especially necessary in that opera-
tion. The different materials are glass, metal, earthen ware.

Glass, when good, possesses the two first properties in suf-
ficient perfection. It is transparent, and not easily touched
by corrosive substances. But glass vessels neither bear sud-
den alterations of heat without breaking, nor are they strong,
nor do they endure a strong heat without melting. The most

troublesome imperfection is that inability to support sudden alterations of heat and cold, which they have in common with all brittle bodies, and which was formerly explained. The way to diminish this imperfection in glass vessels, is to make glasses which are to endure heat as thin as is consistent with moderate strength. Thus, the surfaces will be more equally heated, and the whole glass more flexible, so that the one half may be heated, before the other with less danger. Florence flasks are extremely serviceable in such operations, being so thin, and of a glass that is remarkably strong. It is a great advantage too, to be spherical where the heat is applied, because this form is easiest made thin, and equally so; and besides, this form receives heat in a more equal and diffusive manner. It is necessary too that the glass be well annealed. By annealing glass, is meant the exposing it, while glowing hot, to a red heat, which is allowed gradually to waste. While glass is of a low red heat, it has a great degree of soft toughness, will bear the blow of a hammer, and be dimpled rather than break. But if a thick glass be exposed in this hot state to cool air, the outer surface cools and contracts, and the softness of the internal parts enables them to give way to this contraction, like a ductile metal, and the glass grows a little thicker. When the outer surface has thus grown cold and hard, the inside commonly splits by contracting. If it remain whole, all the parts are on the stretch, and, in this state, a slight touch of flint, hard sand, or even hard steel, tears this stretched surface with facility, and the sudden shock makes the vibration extend even to the outside, and all flies in pieces.

To remove this imperfection, the glass is made to cool extremely slowly, so that all may contract alike, and take the regular and natural arrangement of the parts. The workmen never anneal funnels, because they say that the inside cools as fast as the outside.

When thus constructed, glass vessels are sufficiently secure provided we be cautious to heat or cool them gradually, and they are useful in a great variety of operations. They are

always preferred indeed where great heats or sudden changes are not necessary, and where strength is not required.

Metallic vessels have the third and fourth qualities in perfection, which glass wants, viz. to bear sudden vicissitudes, and to be strong.   But they are deficient in all the rest, for they are not transparent ; they are liable to be corroded, and dissolved by many active substances ; nor do they support a violent heat, without melting, or being destroyed by it.   The most troublesome imperfection that attends them is that they are so liable to be corroded or rusted, and to taint the subjects put into them.   This is unfortunately the case in such metals as are most usually employed, namely, iron or copper.   They are in some measure improved by tinning, but imperfectly. Besides iron and copper in common use, lead and tin are used too in some particular operations, when a very moderate heat only is necessary, and when acrid substances are treated ; as in the manufacture of alum, or copperas, because they are less liable to corrosion and rusting.   For very nice purposes, when heat is necessary, and all neatness and cleanness must be studied with scrupulous care, as in drying the gold after the depart, vessels of platinum are used.

Earthen ware is the third and last matter employed in the construction of chemical vessels ; and the best kinds of it possess some of the desirable qualities in a high degree of perfection, especially the power to withstand violent heats, without melting, or otherwise suffering.   The basis, in general, is clay, which, when good, has properties that render it very convenient and useful for the formation of vessels.   These properties are ductillity when moist, so as to be easily wrought into any form ; and density, solidity, firmness, are acquired by burning ; while, at the same time, it endures the most violent heat without further change.   These are the qualities of the very pure kinds, as pipe-clay, stourbridge. &c.   Others are impure and melt or vitrify.   Of clay alone, very dense and close vessels may be obtained, with sufficient heat ; but they are extremely liable to crack.   This is more or less prevented by mixing the clay with substances that do not rarefy much by heat, and that divide, or render it more porous. Sand is a pro

per substance, taking care that it be of a kind which does not dispose the pure clay to vitrification. Powdered flint, or quartz, are the best; also the fragments of clay vessels, which have already served, and become hard in the fire, are beaten into a coarse powder for the same purpose. Black lead is one of the finest ingredients for this mixture: but it makes a porous composition, which some melted matters readily soak through. A greater or larger quantity must be mixed according to the purposes intended. (Vide *Pott Berlin Mem.*) Where the most perfect closeness of texture is required, porcelain is chosen. This is a composition, and will be described afterwards; as will also Reaumur's porcelain, when we consider the earths as subjects of chemical examination. .

For some very particular purposes, but of frequent occurrence, the chemist makes use of vessels, called COPLES, formed of bone-ashes.

The variety of vessels, in point of form, depends also on the uses for which they are intended. To give a summary account of these uses, I shall first remark, that, in the conduct of chemical experiments and operations, we have occasion, either, 1*st*, To mix fluids with one another, or with solid substances, and often to promote their union and action on one another by moderate heat; or 2*dly*, To melt solids together with a strong heat; or, 3*dly*, To convert the whole or a part of the subject we are working upon into vapour.

One of these three modes of operation is practised in every chemical process or operation; and all the vessels we employ in our operations are fitted by their form to one of these three intentions. We may therefore divide them into,

1*st*. Vessels for making mixtures and solutions.

2*dly*. Vessels for fusions.

3*dly*. Vessels for vaporations.

These divisions comprehend all the vessels employed in the operations of chemistry. There is one other set of vessels, however, necessary to the chemist, and these are,

4*thly*. Vessels for the preservation of his materials and products.

First, therefore, of the vessels for making mixtures and solutions. There is a little variety in the forms of these vessels, in consequence of some diversity in the nature of the subjects, and in the practice of the operations. 1*st*, We have frequent occasion to make mixtures in which there are some ingredients considerably volatile, and part of which would evaporate, were they exposed to the air. It is also proper, in making many mixtures, to promote the action of the ingredients on one another by gentle heat, or by digestion; which digestion cannot be performed in a common open vessel, on account of the evaporation which would take place. Vessels which serve these purposes are, the PHIALA CHEMICA, MATRASS, or BOLTHEAD, or vessels approaching to that form, such as flasks. The propriety of this form is easily seen. Their oblong shape and small orifice prevents the loss by evaporation, and allows a considerable agitation, for promoting the mutual action of the contents, without any risk of spilling. The bulb is made thin, to bear external or internal heat.

But sometimes we apply greater heat to promote the action of the materials; such a heat as raises them to vapours. As these vapours must be condensed, and made to return into the vessel, the chemist uses an apparatus of vessels, called a CIRCULATING APPARATUS, or PELICAN, from its distant resemblance to a bird pecking its breast. The apparatus consists of a tall pear-shaped vessel, for receiving the subject. This has a broad-headed, or turnip-shaped capital, fitted into its mouth. The vapours rise into this head, and are there condensed, and would fall back through the rising vapours, and condense them also; but this is avoided by making two pipes branch out from the lower side of the head, and reach pretty far down, where they are inserted, sloping into the sides of the upright body. Thus, the fluid produced by condensation in the head, runs down by the lateral pipes into the body. This operation is called CIRCULATION.

COHOBATION is merely the pouring back into the body of the matrass the liquor which has been collected, by properly condensing the vapours. Here we do by hand, and from time

to time, what the circulating apparatus performs without our attendance.

In these ways of applying heat to our mixtures, however, we cannot raise it higher than the degree which the materials can bear under the ordinary pressure of the air. I told you formerly that Dr. Papin, an ingenious physician and chemist, has gone much farther, and has contrived a machine, by means of which we can apply to some of these mixtures a much greater heat than they can bear in ordinary circumstances. Such is his DIGESTER formerly mentioned. In this machine the operation called DIGESTION is pushed to the greatest heat possible. You are doubtless aware that the operations with Papin's digester must always be conducted with caution, that accidents may not happen by the vessel's bursting. The di-gester must be made of copper, soldered with hard solder, and its lid must be provided with a loaded valve, which will allow the vapour to escape when its elasticity becomes hazardous. By loading the valve at the rate of ten, twenty, thirty, or forty pounds on a square inch of the surface of the valve, we shall raise the heat of watery mixtures to about 236°, 254°, 270°, and 284°, of Fahrenheit's scale. By managing the fire so that the valve only puffs now and then, under these loads, we shall know pretty nearly the heat of the operation. The digester has long been used in Germany for the purposes of cookery, such as for making strong broths from bones and sinews and the less delicate parts of meat.

There is another operation, in which, on account of the na-ture of the materials, which are less volatile, a still greater heat is applied to make them act. The materials are laid *stratum super stratum*, and pressed close together, into vessels of a proper form, made of earthen ware, or other materials which will stand the red heat which is necessary for converting one of the substances into vapour, in which state only it is able to penetrate and act upon the other in the manner we want. This is a process chiefly practised in metallurgy. From some resemblance between the arrangement of the materials, and the laying of bricks in mortar, this practice has been called CEMENTATION.

Other instruments employed by the chemist to facilitate mixture and solution, are the mechanical instruments by which solid substances are broken into fragments, or pounded to powder, to prepare them for being dissolved or mixed with others ; or, if they are malleable, they are beaten into thin plates, or reduced to filings ; all which is effected by means of mortars, sieves, files, the hammer and anvil, &c.

In the last place, as it often happens that the matter we are employed in dissolving does not dissolve completely, but that a sediment, or fæculency, or other undissolved matter remains we purify the solution by filtration or by subsidence.

We must next consider the vessels employed in the operations in which solid materials are melted together with a strong heat. These are CRUCIBLES, which, by their taper form, and the triangular form of the brim, are fitted for precipitation, and for pouring out. Besides the mere crucible, we must have covers, to keep out the coals or ashes. A smaller crucible, inverted on the mouth of that containing the materials, is a very convenient cover. They must generally be set on pedestals of considerable height, that there may be abundance of glowing coals below as well as around them. They are lifted, for pouring, with a pair of crucible tongs, the ends of which are bent into a shape fitted for grasping the crucible, and holding it steadily. The contents are poured out into a mould, called an INGOT-MOULD, or into a conical brass vessel, shaped like and old-fashioned beer glass. A common small brass mortar will answer the purpose very well. These moulds are rubbed with chalk, or are smoked, to prevent adhesion.

The materials of crucibles are always earthy compositions, made with great care, as they are often exposed to great heats. There are but two varieties in common use....the Hessian, and the Ipsian, or Austrian, or black lead. The Hessian are manufactured in immense numbers in the neighbourhood of Cassel. They are composed of a very fine clay, mixed with a great proportion of sand, and are baked to a considerable degree of hardness. They stand a most violent heat without vitrification. Their great excellency is their compactness, not

easily penetrated by the melted matters. Their defect, con-
nected with this compactness, is a readiness to split, when a
vein of colder air sometimes gets at them while they are
intensely hot. The Austrian crucibles, composed of clay
mixed with black lead, can scarcely be acted on by any ma-
terials contained in them; and they bear sudden cooling
much better than the Hessian. But they are so porous, that
some metalline glasses and flags soak through them like water.
They are of most use for melting the most refractory metals.
The ordinary method of preventing them from cracking is by
doubling them, that is, setting one within another, with pow-
dered clay between, or by luting without, with a coating of
clay and sand.

These vessels are employed for fusion, or for operations
that depend on it, when the heat required is considerable.
For sometimes others will do, as iron, for melting lead; glass,
for some salts, &c.

We must next attend to vessels contrived for containing those
bodies, or mixtures of bodies, which are to be converted in
whole, or in part, into vapour.

I had lately an opportuniy of remarking, that in operations
of this kind, we have one of two ends in view. We either
wish to dissipate the volatile parts, and to preserve the fixed only;
or we desire to preserve the volatile parts. The operation we per-
form, is, therefore, different in these two cases, and is called
in the first *evaporation*, and in the second *distillation*, or *subli-
mation*. In evaporation, the vessels are open; in the other
two, the vapour is confined, and directed from the vessel in
which it is formed into another, which is kept cool, where it
is condensed again. This is called DISTILLATION, when the
vapours condense into a fluid: and SUBLIMATION, when they
condense into a solid matter. The vessels employed in all oper-
ations in which vapour is produced, may therefore be arrang-
ed into those fitted for evaporations, and those fitted for dis-
tillations or sublimations.

The vessels employed for evaporations are open vessels,
the materials and form may be different according to the nature
of the subject. If it is a mixture of water with other matters

which have no corrosive quality with respect to metals, any metallic pan or pot, or other vessel, may serve. If the mixture be of a corrosive nature, the vessels must be of glass or earthen ware. The best form of glass vessels for evaporations, is the globular; and, where there is an opportunity, they may be improved by hardening them in Mr. Reaumur's way. Or we may use vessels of the best and hardest kinds of earthen ware, excellent ones for this purpose are made by Mr. Wedgewood.

There are some cases of evaporation, in which the heat must be exactly regulated. There are some substances which suffer from a brisk heat, as extracts, juices, or decoctions of plants; and which it is therefore proper to evaporate with as gentle warmth as possible, by insensible or spontaneous eva_poration. If the utmost care be not taken, the extract, when brought to its proper degree of concentration, is generally black, and has a burnt taste, even although the heat appears far below what would colour any vegetable substance. Here we must make amends for the mildness of the heat, and promote the evaporation, by spreading out such matters in as broad a surface as possible. We therefore use, in this case, large flat dishes of stone-ware, or porcelain; and the best way of using these is to place them in the mouth of a copper or iron vessel containing water. Thus, the steam of the water will heat them equally, and secure them from breaking, and the heat will never rise too high. This manner of applying heat is called the *balneum vaporis*. When the vessel is set in boiling water, it is called *balneum mariæ*, and when in sand, it is called *bal-neum arenæ*.

In some cases of evaporation, when the heats required to produce vapour are very strong, and metallic vessels are im-proper, we use crucibles, as in evaporating some metallic sub_stances themselves.

To these particulars, with respect to the conduct of evapo-ration, we may add a method proposed not long ago by Dr. Hales, for performing evaporation more quickly with a gentle heat. This is by blowing up showers of air, as he expresses himself, through the liquor. This he proposed, in order to

hasten the distillation of sea water; and he found it effectual for discharging certain bad smells or flavours from milk, &c. An instrument of this kind is sold in London, to sweeten stinking water at sea, which it does, by evaporating the putrid steams. The instrument is merely a long pipe, terminating in a broad streamer, like that of a garden watering pot. This is joined to the nozzle of a pair of bellows, and the streamer is immersed to the bottom of the fluid, and the bellows are worked, to force the air through the holes, in a great number of small streams. This contrivance was applied, on a very great scale, to the evaporation of water, in the manufacture of sea salt; but, instead of promoting the evaporation, it was found to check it prodigiously. It must do so, because, in this manufacture, a boiling heat is employed; and cold air showered through the boiling water, must both cool the water, and condense the steam. When we come to consider this manufacture, we shall mention much more effectual methods of applying the abstracting power of air.

Let us next consider the vessels employed in distillation and sublimation. I said just now that we give these names to the operations in which we direct vapour into another vessel, or another part of the vessel, in which it is condensed again by cold, and that the distinction is taken from the appearance of the condensed matter; if fluid, distillation; if solid, sublimation. I may also explain here a few more terms. The term, SPIRIT, is often applied to the fluids obtained by distillation; and the terms, SUBLIMATE, or FLOWERS, to the matters condensed in sublimation; the more fixed matter which remains behind being commonly, in both cases, called the RESIDUUM, and sometimes CAPUT MORTUUM; when solid and black.

To return to the vessels....Those employed in distillations have been suited to three different ways in which this operation has been conducted; and these three different ways got the names of distillation; 1. *per descensum*, or downwards 2. *per ascensum*, or upwards; 3. *ad latus*, to a side.

The first is, when the heat is applied above the subject, and the vapour is forced directly downwards, and condensed in a part of the vessel, kept cool below. See in chemical books,

the manner of obtaining oil of cloves in this way. But it is laid aside, and there is no operation of this kind in use at present, except the preparation or manufacture of tar, which resembles this, and the separation of zinc and mercury from the ore.

Distillatio per ascensum, is where the vapour is allowed to follow its natural disposition, and to ascend to some height upwards from the subject, and is then directed aside into a cold cavity, where it is condensed. For this a variety of vessels have been contrived, the most common of which is the common copper still, so well known that it needs little description. It consists of a BODY, a HEAD, and a REFRIGERATORY, or cooler. From the head issues a tube, inserted into the upper end of another tube, which passes obliquely downwards through the refrigeratory, but without communicating with it. This last tube is often formed into a cylindrical spiral, like a cork-screw, in order to increase the quantity of cold surface, to which the passing vapour may be applied for condensation. The liquor produced trickles down the sloping spiral, and is received into a proper vessel. Some have them with different heads; and it is also common to have a water-bath-still adapted to the common one: that is, the body of the still is set into another body, distant from it a little way all round. Water is kept boiling in the outer vessel, and then a most regular and uniform heat is applied to the substances contained in the inner body. This still answers for the distillation of all such subjects as are not disposed to dissolve or corrode the metal still, or worm. But when we have corrosive acrid substances to deal with, or when we have a small quantity of any matter, we use glass vessels. And those that resemble the common still most in form, are CUCURBITS, with alembics or capitals, and a receiver. Cucurbits are called BODIES by the English chemists. These are made higher or lower. The two joints must be made tight, by lute, or slips of linen smeared with clammy matter, and then lapped round, over the joints.

But when glass vessels are proper, either on account of the acrimony of the subject, or its small quantity, or our desire

to see the phenomena, the vessels most simple, and generally most convenient, and therefore most commonly used, are the RETORT and RECEIVER. The distillation performed in these is the *distillatio ad latus*. It is more convenient in the greatest number of cases, because there is only one joint. Retorts are made of different heights, and may be placed differently. The materials are commonly introduced into retorts by a funnel. But for some operations, it is proper to have tubulated retorts; that is, having a tube formed at top, through which the subjects can be poured. The tube is then shut by a cork, or a ground glass stopper.

Glass retorts serve for a great variety of distillations, where moderate heat is sufficient. If a stronger heat be requisite, the retorts must be coated with clay, which helps to prevent both breaking and collapsing; or they must be used of earthen ware, like that of crucibles. Green glass coated is better. For particular purposes, it is convenient to have a sort of iron retort, of two pieces.

The ordinary form of receivers has been globular, but a conical form, ending with a hemisphere, is better. Sometimes a hole is bored in the neck, that the fumes may escape, which would otherwise burst the vessel. Some receivers have a tube joined to their bottom. These are called spout receivers. In operations upon some very volatile substances, the older chemists imagined that a simple receiver was not sufficient, and they used a series of them, each of which was terminated in a wide tube, which fitted into the neck of the next. Such were called ADOPTERS. They are never used now, and a large receiver is much better. In very difficult cases of this kind, some of these intermediate vessels are found indispensably necessary. Mr. Peter Woulfe, F. R. S. has contrived an apparatus for those difficult distillations, which, by proper modifications, will obviate every difficulty. A particular description of this apparatus would take up much of our time, and therefore I refer you to Phil. Trans. vol. 57. p. 517.

After, or during some distillations, we have occasion to make use of a SEPARATORY, which is a glass vessel, of a tall

form, having a spout rising from nearly the bottom, and rather exceeding the vessel in height. It is plain that by this contrivance, the lower parts can be poured off, without disturbing or being disturbed by the upper parts.

For sublimation, the vessels employed are sometimes the cucurbit and capital, because, in many sublimations, some liquor comes before the dry matter sublimes. But in such cases, we more commonly use a retort with a wide and short neck and a receiver; for which purpose the conical form is much the best. For particular purposes we use an oblong or spherical glass vessel,....a Florentine flask, or even a common phial; and sometimes the aludels, or allodials, which resemble the antique receivers which are mentioned above, are used in a series.

Having now finished the list of vessels which are employed in chemical operations, there only remain to be mentioned those necessary for the preservation of many of the products of chemistry. Many of those products are volatile, and require to be shut up closely in vessels. Others are affected by the humidity of the air; and it happens, at the same time, that many of them are very corrosive, and destroy common corks. Such can be preserved, therefore, only in glass vessels, with glass stoppers, that shut them close; and a number of such phials are necessary.

It is also proper to subjoin to the enumeration of the vessels, the mention of a means by which we alter them on some occasions, by cutting off parts of them to fit them better to our purpose. This is done by an iron knife or rod made red hot. A small scratch being first made on the edge of the glass, the red hot knife is applied there, and presently splits the glass in that part. By keeping the rod applied, and turning the glass very slowly under it, this crack may be conducted wherever we please, and the whole glass may be reduced to a narrow spiral ribbon. To cut thick bottle glass quite round, we may tie round it four threads of cotton-wick, and wet it with oil of turpentine, and set it on fire; and when it is burnt out, sprinkle water on it, and it will separate immediately.

Having given you such remarks as may serve to direct you in the choice of the vessels employed in chemical operations and experiments, we pass to the second part of this section, namely, the means for producing, and the way of managing the heats which are required in those operations.

The means which the chemists, as well as others, employ for producing heat upon ordinary, and almost all occasions, is the inflammation of combustible bodies, or the burning of fuel. But as there are several other sources of heat beside this, most of which have been used occasionally for particular purposes, and some of which may be capable of producing singular effects ; I shall enumerate them shortly, and just notice what are the purposes to which they have been applied, or which they may be capable of answering in chemistry.

The means by which various degrees of sensible heat are produced and communicated are, I think, seven :

1. That principle or power in animals which keeps them warm, and enables them to communicate heat to other bodies.

2. Friction or percussion of hard bodies.

3. Electricity.

4. Mixture.

5. Fermentation or putrefaction.

6. The sun's rays.

7. Inflammation of fuel.

1. The heat produced by the first of these causes is employed for no chemical purpose, except the regulation of thermometers. The heat of the person who makes them being always the same while in health, he can find this degree on a new thermometer, by putting it in his mouth for some time.

2. Friction or percussion of hard bodies is applied to some purposes in the arts and in common life ; as for kindling gunpowder in the use of fire-arms, and to kindle fuel according to the Indian practice ; but it is seldom used by the chemists except occasionally for kindling fuel*.

* The experiments of Mr. Pictet of Geneva, on the heat produced by moderate friction in the air and *in vacuo*, are curious, and promise to lead

3. The third means I mentioned, electricity, produces the electrical fire or flash which appears when the electrical fluid is greatly condensed in its motion from one place to another. We thus produce very violent heats....heats perhaps superior in violence and intensity to any other whatever. This appears from their effects, 'such as melting gold, and iron, and other metals in a moment, and even dissipating them, or converting them into vapour, which penetrates the surface of the glass on which the experiment is made. But we can produce these heats by electricity, in such quantity only as is sufficient for heating very small masses of matter. The reason why we succeed with these is, that the heat is produced not gradually, but instantaneously; so instantaneously, that there is not time for any part of it to be lost by being communicated to other bodies before it produce its effect. Hence money has been melted in the pocket, and a sword in the scabbard. Dr. Lind melted steel under water by an electric flash.

Such facts once suggested the notion that the fusion occasioned by electricity was a cold fusion; but this is a mistake. A wire too thick to be melted may be made red hot. Electricity, therefore, is a most powerful means for producing heat, and produces it in a very particular manner. However, as chemists have not much attended to these particulars, merely as sources of heat, the instruments for exciting electricity would be improperly considered as a part of the chemical apparatus.

4. The fourth means of producing heat is the mixing particular bodies. This is not applied to use in the practice of chemistry for producing heats, because the heats produced in

to some further knowledge of heat. He published an account of them in the Journal de Physique, and also in a small volume, Geneva, 1790.

Also the great experiments made by Count Rumford on the production of heat by the friction of metals, are peculiarly valuable, and lead to much speculation concerning heat. See Phil. Trans. 1798.

It is worthy of notice that this production of heat is accompanied with light, even in cases where it cannot be ascribed to combustion, as in the collision or friction of pieces of quartz, and other siliceous and hard bodies.

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this way are all momentary and transient. Some of them are used by the fire-eaters and raree-show-men for suddenly kindling tow, &c. They will be mentioned in the course of our examination of chemical substances.

5. The fifth means I mentioned as sometimes employed, is by taking advantage of the heat generated in the fermentation or putrefaction of vegetable and animal substances. Fermentation and putrefaction are changes to which animal and vegetable substances are liable, when preserved from excessive cold, and tempered with a moderate quantity of moisture. These changes come on gradually, and require some time to complete them. During this time, the contexture of the substances is destroyed, their principles enter into new combinations, and the ostensible chemical properties of the matter are quite changed. While this change goes on, there is a constant emission of heat from the mass, which can therefore be employed to communicate this heat to other bodies. The heat produced in this way is not strong, but very lasting, and cheaply procured. Hence it is used for promoting vegetation ; and, in Egypt, it has been long in use for the hatching of eggs, in particular parts of the country where they rear incredible numbers of fowls (Vide Reaumur). And in chemistry, it has been often employed for the digestion of various mixtures, to which purpose it is remarkably adapted. In some chemical arts too, the heat thus produced is employed as a digesting heat, as in the manufacture of verdigrease and of white lead.

The subjects which have been commonly used are horse dung, and the oak bark of the tanners, either of which is capable of maintaining a heat equal to 120° Fahrenheit or more. The first of these acquires this heat soonest, and is capable of becoming hotter than the other ; but the intestine motion, and the generation of heat is sooner over, commonly in about a month. The heat produced by the putrefaction of bark is more gentle, but more lasting, so as to continue, if properly managed, for some months. The particulars to be attended to in using this means of producing heat, are, first, that there be a proper degree of moisture. If there be either too much

or too little, the fermentation will not go on properly, and little or no heat will be produced. Secondly, that a proper quantity of materials be gathered together in one compact heap, The reason why a small quantity, or a heap that is not compact, will not answer the purpose, is that the surface of the mass, which is in contact with the surrounding air, or other cold bodies, bears so large a proportion to the contents, that the heat is carried off as fast as it is produced within. But if the heap be considerable, and properly compacted, the dissipation and waste of the heat is diminished, and it is accumulated in greater quantity in the fermenting materials. In some cases, when very large quantities of tender vegetable substances are heaped up together, and a due degree of moisture conspires to promote this intestine motion, the waste of heat is so small. and its extrication so copious and quick, that the mass is consumed by it, as is well known to have often happened to ricks of hay, when put up too green or wet, so that they contained moisture enough to admit of putrefaction.

6. The heat of the sun's rays. These, when a subject is fully exposed to them in a sheltered situation, give a heat that is not inconsiderable, equal even to that which is commonly produced by putrefaction. *(See note 25, at the end of the volume.)* The heat of the sun's rays may be usefully employed in digestions, &c. but it is not so constant as that obtained by putrefaction. As the heat thus produced is not attended by humid steams, it is useful also in drying many bodies, or dissipating their moisture.

The rays of the sun have a particular effect upon many coloured substances, in discharging the colour. They are a severe trial of dyes. Moreover, some bodies are remarkably changed in their composition by being exposed to the light of the sun.

Such are the effects of the rays of the sun, in their ordinary state ; but they are remarkable on account of the amazing violence and intensity to which we can increase their effect, by condensing them, or directing a great number to fall upon the same spot. This is effected by concave mirrors, or convex lenses. Instruments of both these kinds have

been executed, at different times, which produced effects that appeared astonishing when they were new; such as melting in a moment many earths and stones, which were reckoned before perfectly unfusible; converting into smoke, gold, silver, and other metals, which were imagined before to be perfectly fixed. See some of those facts related by Boerhaave, and in the Memoirs of the Academy of Sciences of Paris. The most famous of those instruments are a speculum, made by the Viletti of Lyons, a father and two sons, and a lense, made by the celebrated Baron von Tchirnhaus*. The diameter of the speculum is 43 inches, the diameter of the focus half an inch, the distance three feet, its weight 400 pounds French. The lens belonged to the regent Duke of Orleans, and with it were made the experiments related by Homberg and Geoffroy, in the Memoirs of the Academy. The diameter of it is 33 inches, the focus at the distance of 12 feet, and the diameter of the focus $1\frac{1}{3}$ inch. The effects of the speculum were, however, much greater than those of the lens, and for this plain reason, that the image of the sun is formed by it at a small distance, and therefore not so broad, and the rays are therefore condensed into less space. For this reason speculums have more effect, in general, than lenses†. Specula of different sizes are not so different from one another in power as we might imagine. This will not appear surprising, when we consider that, supposing the exactness of workmanship equal, the rays will be equally condensed by every speculum. The diameter or breadth of a

* Both of these ways of kindling fire were known to the ancients. Euclid, in his Catoptrics, mentions expressly this effect of a spherical speculum, only mistakes the situation of the focus. Plutarch also, in his life of Numa, says that the vestal virgins kindled their fires *Scaphiis*. The stratagem of Strepsiades, in Aristophane's comedy, for burning the documents of his debts, by a globe of glass filled with water, gathering the sun's rays on them, is a proof of the other point.

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† Lenses are inferior, for another reason. They absorb or intercept a great proportion of the heat. This is evident from Scheele's experiments.
                                                                    EDITOR.

speculum is always proportioned to the sphere on which it is ground. A spherical surface collects the light in the focus, only in so far as it coincides with the apex of a paraboloïd, and it happens to be but a small portion of a sphere that coincides with it. The speculum is most powerful....the lens most convenient, because the speculum directs the rays upwards towards the sun. The object cannot be so conveniently exposed to them in that situation, and it interrupts light. It is usual with lenses, to employ another lens, of $\frac{1}{3}$ of the diameter and focal distance, to collect the rays still more. Thus the most intense heats are produced.

There are, in the Memoirs of the Academy, papers upon both these kinds of instruments, and many projects are proposed, and experiments related, with a view to construct them with greater facility and perfection, by Mr. Buffon. This gentleman may be said to have reinvented the contrivance put in practice by Archimedes, when he set fire to the ships of the enemy's fleet, employed in the siege of Syracuse, (*Vide Encyclopedie.*) This story was long considered as one of those marvellous fables which are related of the great men of remote antiquity. But the experiments of M. Buffon have shewn that the thing is possible ; and Tzetzes, a Greek author, describes the contrivance of Archimedes, which proves exactly the same as the invention of Mr. Buffon*. I call it invention, as I do not suppose he was acquainted with the Greek author, otherwise he would have acknowledged the hint he got from him. Mr. Buffon

* Zonaras mentions another instance of this contrivance. Proclus burnt the ships of Vitellianus, at Constantinople, in the same way. This was in the year 514. Kircher undertook to prove the truth of those ancient stories by experiment, and, following the description by Tzetzes, of plain mirrors turning on hinges, (γιγγλυμοις) he produced a very great heat by five mirrors, which he made to unite their reflection at 100 feet distance. Vitellio quotes a passage from Authemius, the architect of Justinian, mentioning his having kindled wood by 24 mirrors. This work of Authemius is now lost, and never has been printed. Vitellio had no inducement whatever to falsify.

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employed a great number of small glass speculums, of 6, 7, and 8 inches broad, framing them so together that their separate reflections united at a convenient distance. With 40, he kindled deal boards at 66 feet distance, and with 128, he did this at the distance of 150 feet. With 45, he melted tin at 20 feet distance, and with 117, he melted silver. At another time he kindled wood at 200 feet, melted tin at 150, lead at 130, and silver at 60, employing 168 mirrors. His contrivance had the eminent advantage of directing the light downwards, which made all experiments easy.

7 We are next to consider fuel, as the most easy and manageable means of producing heat. We can easily manage this source of heat, so as to have all the different heats under command, from the most mild and gentle, to one approaching, in force, to that occasioned by the collected rays of the sun.

This variety in the effect of fuel depends upon the various nature of the fuel itself, and upon the management or command of its inflammation. With regard to the variety among fuels, it is very great. But we shall notice only the most remarkable differences among the fuels commonly used. They may be reduced to five divisions. The first may comprehend the fluid inflammables; to the second we may refer peat or turf; to the third charcoal of wood; to the fourth pit-coal charred; and to the fifth wood, or pit-coal, in a crude state, and capable of yielding a copious and bright flame.

1st. The fluid inflammables are considered as distinct from the solid, on this account, that they are capable of burning upon a wick, and become, in this way, the most manageable sources of heat; though, on account of their price, they are never employed for producing it in great quantity, and are only used when a gentle degree, or a small quantity of heat, is sufficient. The species which belong to this class are spirit of wine, and different oils.

The first of these, spirit of wine, when pure and free of water, is as convenient and manageable a fuel for producing moderate or gentle heats as can be desired. Its flame is perfectly clean, and free from any kind of soot ; it can easily be made to burn slower or faster, and to produce less or more heat, by changing the size or number of the wicks upon which it burns ; for, as long as these are fed with spirit, in a proper manner, they continue to yield flame of precisely the same strength. The cotton, or other materials of which the wick is composed, is not scorched or consumed in the least, because the spirit with which it is constantly soaked, is incapable of becoming hotter than 174° Fahrenheit, which is considerably below the heat of boiling water. It is only the vapour that arises from it which is hotter, and this too only in its outer parts that are most remote from the wick, and where only the inflammation is going on, in consequence of communication and contact with the air. At the same time, as the spirit is totally volatile, it does not leave any fixed matter, which, by being accumulated on the wick, might render it foul, and fill up its pores. The wick, therefore, continues to imbibe the spirit as freely after some time as it did at the first. These are the qualities of pure spirit of wine as a fuel. But these qualities belong only to a spirit that is very pure. If, on the contrary, the spirit is weak, or contain water, the water, being less volatile, does not evaporate so fast from the wick, as the more spiritous part ; and the wick becomes, after some time, so much soaked with water, that it does not imbibe the spirit properly. The flame becomes much weaker, or is altogether extinguished. When spirit of wine is used as a fuel, therefore, it ought to be made as strong, or free from water as possible.

Oil, although fluid like spirit of wine, and capable of burning in a similar manner, is not so convenient in many respects. It is disposed to emit soot ; and this, applying itself to the bottom of the vessel exposed to it, and increasing in thickness, forms by degrees, a soft and spongy medium, through which heat is not so freely and quickly transmitted. This was observed by Muschenbroeck, in his experiments upon the

expansions of metalline rods heated by lamps. It is true that we can prevent this entirely, by using very small wicks, and increasing the number, if necessary, to produce the heat required. Or, we may employ one of those lamps, in which a stream of air is allowed to rise through the middle of the flame, or to pass over its surface with such velocity as to produce a more complete inflammation than ordinary. But we shall be as much embarrassed in another way, for the oils commonly used, being capable of assuming a heat greatly above that of boiling water, scorch and burn the wick, and change its texture, so that it does not imbibe the oil so fast as before. Some have attempted a remedy, by making the wick of incombustible materials, as asbestos, or wire : but still, as the oil does not totally evaporate, but leaves a small quantity of gross fixed carbonaceous matter, this, constantly accumulating, clogs the wick to such a degree, that the oil cannot ascend, the flames become weaker, and, in some cases, are entirely extinguished. There is, however, a difference among the different oils in this respect; some being more totally volatile and inflammable than others. But the best are troublesome in this way, and the only remedy is to change the wicks often, though we can hardly do this and be sure of keeping always an equal flame.

The second kind of fuel mentioned, peat, is so spongy, that, compared with the more solid fuels, it is unfit to be employed for producing very strong heats. It is too bulky for this.... We cannot put into a furnace, at a time, a quantity that corresponds with the quick consumption that must necessarily go on when the heat is violent. There is no doubt a great difference in this respect among different kinds of this fuel; but this is the general character of it. However, when we desire to produce and keep up, by means of cheap fuel, an extremely mild gentle heat, we can hardly use any thing better than peat. But it is best to have it previously charred, that is, scorched, or burnt to black coal. The advantages gained by charring it will be presently explained. When prepared for use in that manner, it is capable of being made to burn more slowly and gently, or will bear without

being extinguished altogether a greater diminution of the quantity of air with which it is supplied, than any other of the solid fuels.   Dr. Boerhaave found it extremely convenient and manageable, in his *Furnus Studiosorum*.

3*d*.   The next fuel in order is the charcoal of wood.   This is prepared by piling up billets of wood into a pyramidal heap, with several spiracles or flues, formed through the pile. Chips and brushwood are put into those below, and the whole is so constructed, that, when kindled, it kindles almost over the whole pile in a very short time. It would burst out into a blaze, and be quickly consumed to ashes, were it not covered all over with earth, or clay, beaten close, leaving openings at all the spiracles. These are carefully watched; and, whenever the white watery smoke, is observed to be succeeded by thin blue and transparent smoke, the hole is immediately stopped....this being the indication of all the watery vapour being gone, and the burning of the true coaly matter commencing.   Thus is a pretty strong red heat raised through the whole mass, and all the volatile matters are dissipated by it, and nothing now remains but the charcoal.   The holes being all stopped in succession, as this change of the smoke is observed, the fire goes out for want of air.   The pile is now allowed to cool....This requires many days: for, charcoal being a very bad conductor of heat, the pile long remains red hot in the centre, and, if opened in this state, would instantly burn with fury.

Small   quantities   may   be   procured   at   any time, by burning wood in close vessels. Little pieces may be very finely prepared, by plunging the wood in lead melted and red hot.

This is the chief fuel used by the chemists abroad, and has many good properties.   It kindles quickly, emits few watery or other vapours while burning, and when consumed, leaves few ashes, and those very light.   They are, therefore, easily blown away so that the fire continues open, or pervious to the current of air, which must pass through it, to keep it burning. This sort of fuel, too, is capable of producing as intense a heat as can be obtained by any; but in those violent heats it is quickly consumed, and needs to be frequently supplied.

4th. Fossil coals charred, called cinders, or coaks, have, in many respects, the same properties as charcoal of wood; as kindling more readily in furnaces than when they are not charred, and not emitting watery, or other gross smoke, while they burn. This sort of charcoal is even greatly superior to the other in some properties. It is a much stronger fuel, or contains the inflammable matter in greater quantity, or in a more condensed state. It is, therefore, consumed much more slowly on all occasions, and particularly when employed for producing intense melting heats. The only inconveniences that attend it, are, that, as it consumes, it leaves much more ashes than the other*, and these much heavier too, which are, therefore, liable to collect in such quantity as to obstruct the free passage of air through the fire ; and further, that when the heat is very intense, these ashes are disposed to melt or vitrify into a tenacious drossy substance, which clogs the grate, the sides of the furnace, and the vessels. This last inconvenience is only troublesome, however, when the heat required is very intense. In ordinary heats, the ashes do not melt, and, though they are more copious and heavy than those of charcoal of wood, they seldom choke up the fire considerably, unless the bars of the grate be too close together.

This fuel, therefore, is preferable, in most cases, to the charcoal of wood, on account of its burning much longer, or giving much more heat before it is consumed. The heat produced by equal quantities by weight of pit coal, wood-charcoal, and wood itself, are nearly in the proportion of 5, 4, and 3. The reason why both these kinds of charcoal are preferred, on most occasions, in experimental chemistry, to the crude wood or fossil coal from which they are produced, is that the crude fuels are deprived, by charring, of a considerable quantity of water, and some other volatile principles, which are evaporated during the

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* I don't know that this is the case with the finest pit-coal. I have seen some of the finest Newcastle coals which did not leave $\frac{1}{50}$th of their weight of ashes, and even these did not seem entirely consumed.

process of charring, in the form of sooty smoke or flame. These volatile parts, while they remain in the fuel, make it unfit (or less fit) for many purposes in chemistry. For, besides obstructing the vents with sooty matter, they require much heat to evaporate them ; and therefore the heat of the furnace in which they are burnt is much diminished and wasted by every addition of fresh fuel, until the fresh fuel is completely inflamed, and restores the heat to its former strength.   But these great and sudden variations of the heat of a furnace are quite inconvenient in most chemical processes.   In the greater number of chemical operations, therefore, it is much more convenient to use charred fuel, than the same fuel in its natural state.

There are, at the same time, some kinds of fossil coal, which are exceptions to what has now been delivered in general.   We meet with some of them that leave a smaller proportion of ashes than others, and the ashes of some are not so liable to melt in violent heats.   There is one species too, such as the Kilkenny coal of Ireland, and which occurs likewise in some parts of this country, that does not contain any sensible quantity of water, or other such volatile principles.   But this may be called a sort of native charcoal.   It has the appearance of ordinary coal, but, when thrown into the fire, does not emit smoke or soot. It merely becomes red, gives a subtile blue flame, and consumes like charcoal; only it lasts surprisingly long, or continues to give heat for a very long time before it is totally consumed.   But it cannot be made to burn so as to produce a gentle heat.   If not in considerable quantity, and violently heated, it is soon extinguished.

In using this kind of fuel, it is proper to be on our guard against the dangerous nature of the burnt air which arises from charcoal of all kinds.   Charcoal burns without visible smoke.   The air arising from it appears to the eye as pure and as clean as common air.   Hence it is much used abroad by those who are studious of neatness and cleanliness in their apartments.   But this very circumstance should make us more watchful against its effects which may prove dangerous, in the highest degree, before we are aware of it.   The air arising from common crude

fuel is no doubt as bad, but the smoke renders it disagreeable before it becomes dangerous. The first sensation is a slight sense of weakness; the limbs seem to require a little attention, to prevent falling. A slight giddiness, accompanied by a distinct feeling of a flush or glow in the face and neck. Soon after, the person becomes drowsy, would sit down, but commonly falls on the floor, insensible of all about him, and breathes strong, snoring as in an apoplexy. If the person is alarmed in time, and escapes into the open air, he is commonly seized with a violent headach, which gradually abates.

But when the effect is completed, as above described, death very soon ensues, unless relief be obtained. There is usually a foaming at the mouth, a great flush or suffusion of blood over the face and neck, and every indication of an oppression of the brain by this accumulation of blood. The most successful treatment is to take off a quantity of blood immediately, and throw cold water on the head repeatedly. A strong stimulus, such as hartshorn, applied to the soles of the feet, has also a very good effect.

To return to the fuels....

The 5th and *last* kind of fuel is wood, or fossil coal, in their crude state, which it is proper to distinguish from the charcoals of the same substances. The difference consists in their giving a copious and bright flame, when plenty of air is admitted to them, in consequence of which they must be considered as fuels very different from charcoal, and adapted to different purposes.

I had occasion formerly to remark, when treating of inflammation, that flame is produced from those substances only, which are either totally volatile when heat is applied to them, or which contain a quantity of combustible matter that is volatile or easily convertible into vapour by heat; and that flame is nothing else but this vapour set on fire, or which becomes inflamed as fast as it arises from the body which affords it.

Of this nature, therefore, is the flame of wood and fossil coal, when they are burnt in their crude state. These fuels contain a quantity of inflammable matter that is volatile, and which, when a moderate and stifled heat is applied to

them, evaporates in the form of oily and sooty va-
pours and smoke, and diminishes the heat instead of in-
creasing it. But if they are exposed to a stronger heat,
and air is freely admitted, the sooty vapours are suddenly
set on fire, or become flame, and continue afterwards to
burn as fast as they arise from the wood or coal, in conse-
quence of which they produce a great heat.

Such is the nature of flame and of flaming fuel; and
from this account of it, you will perceive that it is applica-
ble only to certain purposes, and is unfit for the more com-
mon uses in experimental chemistry. For, in the first
place, the greater number of our experiments and processes
do not require an intense and violent heat, but a moderate
and continued heat, either kept to the same degree for
some time, or slowly and gradually increased. This sort
of heat is easily commanded by means of charred fuel,
which burns faster or slower, according to the quantity of
air allowed to pass through it, and will continue to burn
with a very small quantity of air (but more slowly) as
well as when much air is admitted. Whereas flaming fuel
cannot be managed in this manner. If little air be ad-
mitted, it gives no flame, but sooty vapour, and a diminu-
tion of heat. And if much air be admitted to make those
vapours break out into flame, the heat is too violent.

These flaming fuels, however, have their particular
uses, for which the others are far less proper. For it
is a fact, that flame, when produced in great quantity,
and made to burn violently, by mixing it with a proper
quantity of fresh air, by driving it on the subject, and
throwing it into whirls and eddies, which mix the air with
every part of the hot vapour, gives a most intense heat.
This proceeds from the vaporous nature of flame, and the
perfect miscibility of it with the air. As the immediate
contact and action of air is necessary to the burning of
every combustible body, so the air, when properly applied,
acts with far greater advantage on flame, than on the solid
and fixed inflammable bodies: for when air is applied to
these last, it can only act on their surface, or the particles
of them that are outermost; whereas flame being a va-
pour or elastic fluid, the air, by proper contrivances, can

be intimately mixed with it, and made to act on every part of it, external and internal, at the same time. The great power of flame, which is the consequence of this, does not appear when we try small quantities of it, and allow it to burn quietly, because the air is not intimately mixed with it, but acts only on the outside, and the quantity of burning matter in the surface of a small flame is too small to produce much effect. But when flame is produced in large quantity, and is properly mixed and agitated with air, its power to heat bodies is immensely increased. It is therefore peculiarly proper for heating large quantities of matter to a violent degree, especially if the contact of solid fuel with such matter is inconvenient. Flaming fuel is used for this reason in many operations performed on large quantities of metal, or metallic minerals, in the making of glass, and in the baking or burning of all kinds of earthen ware. The potter's kiln is a cylindrical cavity, filled from the bottom to the top with columns of ware. The only interstices are those that are left between the columns; and the flame, when produced in sufficient quantity, proves a torrent of liquid fire, constantly flowing up through the whole of those interstices, and heats the whole pile in an equal manner.

Flaming fuel is also proper in many works or manufactories in which much fuel is consumed, as in breweries, distilleries, and the like. In such works, it is evidently worth while to contrive the furnaces so that heat may be obtained from the volatile parts of the fuel, as well as from the fixed ; for when this is done, less fuel serves the purpose than would otherwise be necessary. But this is little attended to, or ill understood, in many of those manufactories. It is not uncommon to see vast clouds of black smoke and vapour coming out of their vents. This happens in consequence of their throwing too large a quantity of crude fuel into the furnace at once. The heat is not sufficient to inflame it quickly, and the consequence is a great loss of heat.

These are the principal varieties which occur in fuels, so far as they are fitted to produce more or less violent heat, or for heating bodies in different ways. There are

differences too among them, in consequence of which they produce other changes upon bodies in contact with them, than merely heating them more or less. This depends either on introducing something or taking something away. But I shall notice this when treating of the particular bodies which are disposed to produce effects in this way.

The means of managing and commanding inflammation are now to be considered under the article furnaces.

The necessary requisites of every furnace will occur, if we consider the purpose of the contrivance in general, together with the nature of fuel and inflammation. Our purpose, for instance, in the contrivance of furnaces, is to confine the heat as much as possible to the subject, that it be not wasted, nor prove troublesome to the operator. There is therefore a chamber, or cavity, to contain the burning fuel, called a FIRE-PLACE, the sides of which are of some thickness, and the materials such as withstand the heat. There must likewise be a door, or opening into this chamber, at which fresh fuel may be introduced. And it must not be quite close above ; it must have a VENT towards the upper part, to convey away the smoke and heated burnt air, otherwise the fire would be quickly extinguished. It is next obvious, that any fuel which we can employ in ordinary furnaces must produce ashes, which would collect and choke up the fire. It is necessary, therefore, that the fuel lie on a grate through which the ashes may fall in proportion as they are formed or collected in any quantity. Under the grate it is convenient to have another chamber for receiving these ashes, an ASH-PIT ; and at the side of this chamber, another door to take them out, and likewise to allow fresh air to flow up into the furnace, to supply the place of that which is passing through and going out at the vent. These are necessary parts of every structure called a furnace ; and it is evident, that in such a structure inflammation will go on, or fuel burn, with considerable rapidity. The two requisites to the inflammation of combustible bodies, are a certain degree of heat once excited in the burning body, and a constant renewal of the air in contact with it. The first of these conditions is obtained when the fuel is once

set on fire, seeing that, in burning, it produces a great deal of heat, by which it is both kept hot itself, and communicates heat to the surrounding bodies. The second condition is a necessary consequence of the structure of the furnace, for no sooner is the fire well lighted than there is a motion of the air upwards through the furnace with considerable velocity. The air in the fire-place, by being heated, is expanded, made rarer and lighter, and it is therefore pushed upwards by the cooler and heavier external air, which communicates with it by means of the door of the ash-pit and grate; and thus it will be made to flow in a stream through the vent. Its place is supplied by fresh air rising up through the grate, and fuel. No sooner does this fresh air touch the fuel than it is heated, rarefied, and pushed up in its turn, so that there is a constant stream of air flowing up through the furnace, as long as the fuel lasts or the furnace continues warm. The burning of any inflammable body in any manner whatever, is always attended with an ascent of air like this, and hence the ascent of smoke and flame ; but it is most remarkable in a furnace in which fuel is allowed to burn freely, because the external air, not having admittance to the space above the fire, except through the grate and fire itself, is more heated and rarefied when it has ascended through it, than the air which gets admission above an ordinary fire, which has a free communication with the surrounding air, and is constantly mixing with it. It is plain, that the taller we make the chimney or vent of a furnace, there is a longer column of hot and light air in it, and therefore a greater pressure upwards. Air rushes in more violently by this increased preponderancy of the air of the laboratory ; this consumes more fuel in the same time, and therefore excites a still greater heat. In the iron smelting furnaces, they find the produce nearly in proportion to the number of cubic feet of air thrown into the fuel.

The venting of chimneys depends on the same principles, and the accidents to which it is liable, or bad venting, will be best remedied by a person who understands these principles. Bad venting of chimneys is chiefly of two kinds, proceeding from different causes : 1st, A slow but

constant emission of smoke into the room. 2*d*, Violent puffs of smoke, now and then only, and when particular winds blow, the vent at other times carrying up the smoke well enough. The causes of the first are, 1, Obstruction or narrowness of the vent; 2, Too perfect closeness of the room or house ; 3, Too great wideness of the vent below, which allows too much air to get in above the fire, and thus be less heated ; or, 4, Its being seldom warmed by fire.

The cure of narrow and obstructed vents is obvious. Over-closeness of the room or house, is cured by opening the windows or doors, or making other openings at which the air may enter. A hole above the door of the room is often effectual ; sometimes, however, it is not, namely, when there are well-going vents up stairs. In such cases, the air of the room is disposed to go up stairs to supply them. You will frequently observe that a candle held near the top of the open door indicates a current outwards, while one held near the bottom always shews a current into the room. The most generally effectual method that I know, is to put a slip of wood into the sole of the window, which will keep the sash from shutting close down, but does not allow any air to enter at that place. The air enters by the passage which is always left between the upper and under sashes: but it comes in such a manner as not even to disturb the flame of a candle held close by it. The air flies upwards along the glass, strikes against the soffit of the window, and thence it spreads into the room without offending any person. In some houses, air comes down the vents in which there are no fires, or in which the fires are weak. Hence we are troubled by our neighbour's smoke. Too great wideness of the vent is cured by filling up a part of it at each side of the grate.

The cause of the second kind of bad venting is various, and arises from situation : it can only be cured by what will increase the current up the vent. Contracting the vent below has some effect, by obliging the air of the room to come nearer to the fuel, by which it is more heated and expanded, and is, therefore, driven up the vent with greater force but it is more certainly and completely remedied by something being done above.

A certain degree of this bad venting is sometimes pro-
duced by the bad situation of the fire-place, and door of the
room, with respect to one another. In some cases it is
cured by admitting air into the room at some particular
place or side of it, generally that which corresponds to the
wind which occasions the puffs. Sometimes, however, it
is almost incurable ; as, when the wind beats on a higher
house to leeward. This condenses the air, and it endea-
vours to escape along any protected route ; therefore down
the chimney, and from thence out by the doors and win-
dows.

After having thus formed a clear idea of the ventilation
of a furnace, it will be easy to understand the methods of
regulating the heat. If we want it very moderate, we di-
minish the supply of air, by diminishing the aperture either
above or below. This is called Damping. It is best done
by damping below, viz. by closing the door of the ash-pit,
and admitting the air through holes proportioned to one
another, according to some convenient series, which shall
put it in our power to admit all the intermediate propor-
tions of air. But in order to have holes, the apertures of
which are so proportioned, we must not make the diame-
ters of them in these proportions. This would give aper-
tures widely different from what we want. The areas or
apertures of circles are as the squares of their diameters :
Therefore, having determined on the proportion of the
different holes, we must make their diameters in the pro-
portion of the square roots of those numbers. I always
employ a series of holes in the continual proportion 1, 2, 4,
8, &c. and by using more or fewer of them together, any
quantity of area can be employed. The construction of
this series is exceedingly simple. (Fig. 7.) Let the lines
AB and AC contain an angle BAC, of 45 degrees. Make
AD the smallest intended diameter. Draw, in succes-
sion, DE, EF, FG, GH, HI, IK, &c. alternately perpen-
dicular to AB and AC. Then the area of the hole whose
diameter is AD being 1, the lines AD, AE, AF, AG,
AH, AI, AK, will be the diameters of holes, whose areas
are 1, 2, 4, 8, 16, 32, 64, *(Euclid, 1. 47.)*

When the furnace is very air-tight in the doors and all the joints, and the fuel of an uniform good kind, the consumption of fuel will be perfectly uniform, and in proportion to the area of the open holes. In the same proportion, as far as we can judge, will be the heat that is kept up. But, if the vent of our furnace have a connection with the chimney of the room, it may be greatly affected by its irregularities. It should, therefore, be unconnected with it, standing open under it.

If we want to raise a greater heat, we must open more passages below the fire-place, or add a tube to the vent, to increase its draught. We must never admit the air into the furnace above the fuel. This, by passing between the fuel and the subject, will abate the heat which it receives.

Sometimes the furnace is set within a tall dome. By warming the air within this dome, while the furnace receives its air from below the dome, or independent of it, and the dome is shut on all sides, and open only at the top, it is evident that the levity of the air in the dome must assist the levity of the air in the furnace, and thus increase the draught and the heat. This will not be the case, if the furnace receives its air by its ash-pit, from the dome, nor if there be any opening in the sides of the dome.

We shall now take a view of the variety that is found among chemical furnaces, or at least point out the most useful and remarkable kinds. And, as the vessels were distinguished into three divisions, so the furnaces may, in the same manner, be brought under three similar heads, or divided into furnaces for digestions, fusions, and evaporations.

First, therefore, for digestions, we may occasionally employ several different furnaces. But those which have been particularly contrived for this purpose, are the ATHANOR, with its sand heats, and the lamp, and LAMP-FURNACE. The athanor, or perpetual furnace, consists of a tower, round or square, having below, an ash-pit, grate, and fire-place. The vent goes off laterally immediately above the grate, and is first conducted horizontally, passing under a row of vessels, or baths, for digestions, and then terminates in an upright chimney. A little fire is kindled

on the grate, and the top of the tower being shut, the hot air passes along the horizontal opening, goes up the chimney, rarefies its air, and soon produces a current. The cover is now taken off from the tower, and this is speedily filled to the top with charcoal, and then carefully shut up above. From this construction, it is plain that no fuel can burn for want of air, but what lies immediately on the grate. As it wastes, the charcoal above it subsides, comes into the fire-place, where it burns in its turn, and the heat is kept up as long as it lasts ; and we can at any time lift off the cover, and speedily throw in more charcoal. The lamp, and its furnace, are often used for distillations, and shall therefore be described afterwards.

The second, the MELTING, or WIND-FURNACE, is among the most simple. When complete, it has all the parts that belong to furnaces in general, and no more. The best, for the experiments of the philosophical chemist, are made of plate-iron, well rivetted, and lined with luting. Much has been written about the most effectual forms for producing intense heats, in such small furnaces. They are gravely recommended to be elliptical, having one focus about the bottom of the crucible, and the other near its top ; but these are fancies, no way connected with the nature of heat produced by fuel. A great deal of trouble may be saved, by quitting these fancied forms, and constructing this furnace of a shape that is the most easily executed with exactness. The drawing here given you, is a section of a small air-furnace, and I have found its form and proportions as convenient as any.

The ash-pit A B (fig. 8.) is of plate-iron, cylindrical, and having on one side a door D, a little less incurvated than the cylinder, so that its latch bends it to the sides of the aperture, making it shut and keep very close. Opposite to the door is a register-plate, with a series of holes, each furnished with a brass stopper, nicely fitted to it. On the top of the ash-pit is a flat ring, which carries the grate LK. This being made a little taper, fits exactly into the ring. B E is the body of the furnace, set on the ring of the ash-pit, and the joint made nearly air-tight, by sprinkling the surface of the ring with fine sand, or powdered chalk. The

body is a little wider above than below, and is close all round. On its top stands the dome E F, of a conical form, this joint being also secured with sand or chalk. On one side of the dome is a large hole G, for supplying fuel, and inspecting the crucible. This is stopped with a plug of fire-clay. Two handles must be fixed to the dome for lifting it. It terminates above in a wide tube or vent F H, to which additions may be made, when we would increase the heat. The crucible is to be set on a brick pedestal of such a height that it may be nearly in the middle of the body, and be equally surrounded with fuel in every direction. It should be rather above the middle of the body, because the hot air is all driven upwards, and it receives little heat from the fuel above it. The taper form of the body causes the fuel to collect round it below as it consumes. The ribs of this, and indeed of all furnace-grates, should be triangular, the narrowest side uppermost. This does not allow the cinders to choke the bars, and it gives a steadier position to the pedestal.

If the height B E be 12 inches, the external bottom diameter should be 10 or 11, and the upper one 16. The height of the dome E F should be eight inches. Larger furnaces may be made in the same proportion.

The whole must be lined with a proper lute, (to be described afterwards) to the thickness of two inches. This is made to adhere by means of a great number of nails rivetted to the external plate, having their points curled like so many hooks.

Such, therefore, is the furnace more especially called the melting, or air-furnace, or wind-furnace. The current of air necessary to animate the inflammation to that degree of violence which is often required, is obtained merely by the height of the vent. But there are some cases now and then, when, in chemical experiments, we would desire a rapidity of inflammation, and consequently an intensity of heat, even beyond what this structure can easily be made to produce ; and this is the case still more in many chemical arts, which employ furnaces of such a very large size, that to give them a sufficient height of vent for this purpose, would be very inconvenient and even impractica-

ble. In such cases, we have recourse to mechanical, or other means of impelling the air through the fuel with sufficient velocity. Such are bellows of various construction. When these machines are of considerable size, they are generally wrought by the force of water. But there is another way of employing the force of falling water, to impel air into furnaces or fires, which is much more simple, and is said to produce as great, if not a greater effect. This is by means of the WA-TER-BLAST, which Mr. Lewis describes in his *Commercium Philosophico-Technicum*. The ÆOLIPILE may also be considered as another way of employing water to drive air into fuel, but in a very different manner. The æolipile is merely a globular vessel, having a small pipe coming out from it, something like the pipe of a still, but terminating in a very fine bore. Water is made to boil violently in it by setting it on a chaffer of glowing coals. A strong blast of vapour flows from the pipe. This is directed into the midst of the fuel, which it animates in the same manner as bellows would do; chiefly, I believe, by means of the great quantity of air which it drags along with it from all sides.

After the melting furnace, and other instruments for violent melting heats, the next we shall describe, as being also often employed in melting certain substances, though not with so violent a heat, is called the ASSAY-FURNACE, so called, as it is employed chiefly in the examination of ores. This is not so simple, by much, as the melting furnace. The operations to which it is adapted require that the subject and vessels be prevented from being in contact with the fuel; that on some occasions, it be exposed to a gentle current of air which has not passed through the fuel; and that the operator have free access to see how the changes he proposes to produce are going on. It has therefore an ash-pit and a door like the melting furnace, and a fire-place and door, with a grate in the bottom, through which the ashes fall into the ash-pit. This furnace is not round, but square; and in the middle of the fire-place is fixed a little arched oven, called a MUFFLE, of an oblong form, open before, but shut at the far end. The open end of this oven is made to fit exactly the fire-place door, so as

to appear but one opening; and in looking in, we see nothing but the inside of the muffle. The two sides of this muffle are pierced with several openings like arches standing on its floor. When the fire-place door is opened, the fresh air rushes in, in order to run up through these openings, and thus it plays on the subjects exposed to the violent heat, and burns them completely. This air therefore must be fresh. Above, there is a dome or covering, and a tube.

The effect of this furnace is to produce a considerable degree of heat under the muffle, and a constant stream of air through it; and if we want a stronger heat, and air is not necessary, we close the door of the fire-place altogether.

In using the furnaces hitherto described, the best fuel is charcoal, either of wood or of fossil coal. But I observed formerly, that when violent heats are required, and great quantities of matter are to be lighted, it is better to use flaming fuel, the flame of which, when well mixed and agitated with air, is capable of giving a most intense heat. As an illustration and proof of this last assertion, I shall shew the effects of mixing air with the flame of a candle or lamp by means of the blow-pipe. This little instrument is used in small works on glass, as thermometers, &c. and in soldering metals; it is useful too to the chemist sometimes in adjusting vessels: but, besides, it is also very often used as a very quick and ready method of exciting very intense degrees of heat, when he wishes to examine its effects on a small quantity of matter, such as ores, placed upon a bit of charcoal.

The blow-pipe was recommended by Cramer in his *Ars Docimastica;* but the Swedish chemists were the first who made mnch use of it in examining mineral substances; and one of them, Engestroem, published, at the end of the Translation of Cronsted's Mineralogy into English, an account of the most convenient manner of using it, in which he made some improvements. It is now an indispensable instrument for every mineralogist: and various contrivances have been introduced for accommodating it for an imitation of almost every metallurgical operation. Engestroem describes a box, containing the blow-pipe and other implements, which he calls the Pocket

LABORATORY. The advantages of the blow-pipe are,—it saves a great deal of time and expence, and it enables us to make a multitude of experiments with a small quantity of the materials. The blow-pipe is only fit for heating a small quantity of matter, as the bulk of half a pea, or less. Were we to apply it to larger masses, and to use a wider pipe and much larger flame, it would not produce the effect desired ; the air would pass through the flame without being well mixed with it, and would strike the matter we meant to heat, in such a manner as to cool it, or diminish the heat otherwise produced.

Those who make thermometers and other small instruments of glass with it, often blow it with bellows, but the mouth is most ready and most under command.

Thus we see that flame, mixed and agitated with air, produces a most violent heat. When therefore we want an intense heat, and a great quantity of it, it is best to use flame. And there are furnaces particularly adapted to the use of flaming fuel.

Such is, first, the REVERBERATORY, called by the artists who use it, the AIR-FURNACE. It is used for melting of iron, and for smelting the ores of copper and lead, to extract those metals, and for other purposes.

It has an ash-pit, grate, and fire-place, like other furnaces. The fire-place door is large, and sloping downwards, that the raw fuel may be thrown in more easily in abundance, and gather itself together on the grate. But the flame produced is not allowed to mount straight upwards, but is diverted laterally, by the low arch, into a sort of oven. The floor of this oven is made considerably hollow in the middle, and the matters to be treated are laid there. Opposite to the entry into this oven from the fire-place, is another opening into the chimney or vent. The effect of this construction is plain. The tall vent occasions a current of air through the whole furnace, from the ash-pit door to the top of the chimney. This blows up the fire : and this, by heating the air, increases the current, and the fire soon becomes furious. A volume of flame rises from the raw fuel ; this is hurried into the lateral passage mixed with air ; and the form of that passage causes the stream of flame to dart violently down on the materials lying in the

bed of the laboratory (so is the oven generally named). The low roof of this place, and some inequalities artfully made in it on purpose, confine the stream of flame to the subject, and, by tossing it into violent eddies on its surface, mix the air most intimately with every part of the flame, and thus completely consume the whole combustible matter of the fuel, so that, in a good reverberatory, not a particle of flame goes as far as the bottom of the vent. In the *Journal de Physique*, 1776, there is a description of a Reverberatory Furnace, which seems very ingeniously fitted for a general experimental furnace.

Thus a most intense heat is produced on the matter lying in the bed of the reverberatory. There are openings made in the side of it, just where the matter lies. Here the workmen, from time to time, open the door, and put in their tongs and rakes, and other instruments, to stir and manage the stuff according to their views. Frequently, too, the nozzle of a pair of bellows is directed on the surface of the stuff, to burn it still more completely, and blow the liquid slag and the dross from off its surface to the other side, where a way is made for its getting out.

Another furnace, in which the flame of fuel is employed, is the GLASS FURNACE. The specialities of this depend on this circumstance, that it is necessary for the workmen to have free access to the matter, and, at the same time, to have a violent degree of heat kept up. An ordinary furnace, therefore, with a high vent to produce a draught, and a door, or opening at the side, will not answer; because the cold air would be driven in at this opening, and run up the vent, and thus cool both the fire-place and vent. A glass house, therefore, is so constructed, that there is a high building, of a conical, or other form, over the furnace, which serves as a vent to increase the current of air, and is so wide all round, that there is room for the workmen. The air within this building is heated considerably by the furnace standing in the middle of it, and matters are so disposed, that the current of air thus produced is made to pass chiefly through the furnace, by the ash-pit, which is under the floor of the furnace and DOME, and has a free communication with the external air, but none with the

dome. No more is admitted to come in at the doors of the building than what is merely necessary to render the heat tolerable to the workmen, and to allow them breath. Fig. 9. will illustrate this description. A is the grate for the fuel; B, B, B, the pots; D the holes where the workmen dip their rods into the pots; C the vault of the furnace; and E the ash-pit, communicating with the air without doors. The whole is set in the middle of a vast dome, F. The air coming from the ash-pit through the grate and fuel, ascends through the working holes D into the dome, and thus makes a great column of warm and light air, which rises through the opening of the dome with great force. Some of the holes G open into ovens, in which the materials for glass are roasted, to drive off their volatile parts. Thus it appears, that the working holes are the immediate chimneys or vents of the glass furnace. We cannot otherwise obtain the heat that is necessary. For if there were chimneys, as in other furnaces, the air, rushing in at the work holes, would make it cold in the place where we want the greatest heat. The workmen are therefore exposed to almost intolerable heat. But such heat is not always necessary. Chiefly while the glass is preparing, and in a state like ebullition, much volatile matter is escaping from it. Melted glass being so viscid, it must be made to flow as thin as possible, that these volatile matters may make their way up through it. When the glass is ready for working, they open some of the doors of the dome. This admits the fresh air, in which the men can breathe and work. It abates the heat of the rising column of air in the dome. This abates its ascensional force, and, consequently, the heat of the furnace. This more moderate temperature is called the *settling heat*, the frothing glass being now quiet. After this they increase the heat a little, to bring the glass into a proper state for lifting. This they call the *working heat*.

This, therefore, is the furnace for melting together the materials of glass in large quantities. If our purpose be only to make small quantities, or compositions of different kinds of coloured glasses, or imitations of gems, or experiments of that kind, we may construct such a furnace, as is described by Cra-

mer, at the end of his Art of Essaying, in which he says that
a great number of experiments may be made at the same time
with great ease and convenience.

It remains now only to consider the furnaces for vaporisa-
tion.

The first of these I shall mention is what Macquer calls the
reverberatory, or for performing distillations that require the
strongest heat, in which the vessels employed, commonly of
earthen ware, are exposed to the contact of the fuel ; on ac-
count of which it is commonly called the furnace for distilling
with naked fire.   This furnace is described by Macquer.   It
has nothing very different from the melting furnace, only there
are two openings at the fire-place ; the one is a door for the
fuel ; the other is circular, between the dome and the body,
intended for allowing the neck of an earthen retort to point out.
This hole is formed by making a semi-circular notch in the
upper part of the body, and another in the lower part of the
dome.   When these are set together, the neck of the retort
finds room between them.   If this hole be shut by an earthen
plug, the furnace is the common melting furnace.   Likewise
two iron bars must be placed across the middle of the fire-
place, to support the bottom of the retort ; or, instead of
these bars, we may have a pedestal.   When this vessel is put
in, the opening round its neck should be filled up with luting.
It should have a high vent and dampers.

The second furnace for vaporisation is the furnace of the
common still, having a vent that issues out at one side.

3d, Other distilling furnaces are chiefly fitted for retorts, or
vessels of glass, and are necessary for using these vessels, ap-
plying the heat slowly and equally to the whole surface.   The
particulars in which they differ from others are only contri-
vances for obtaining this end.   Therefore, their upper part
immediately over the fuel is covered by a hemispherical pot of
cast iron of some thickness, in the middle of which the vessel
is placed and surrounded with water, ashes, sand, or other mat-
ters.   Thus, the pot having some thickness and solidity, it
heats slowly and equally, and the matters with which the ves-
sel is surrounded transmit this still more slowly ; and the

whole, being once heated, though the fire may be a little une-
qual, is not subject to sudden variations, which glass-vessels
are unable to bear. The matters interposed between the pot
and the glass vessel have got the name of balnea, or BATHS.
The WATER BATH, *balneum mariæ;* the SAND BATH, *balneum
arenæ.* Iron filings are sometimes used ; others have pro-
posed quicksilver, and melted metals, as tin, lead, and bis-
muth. Sand is the most convenient of any. The older
chemists frequently used no intermedium but air, supporting
the retort on three bits of chalk, and setting over it a little
dome of plate iron, notched, to admit the neck. This they
called distilling *in capellâ vacuâ.* This is a foolish method ;
because, unless the space round the neck of the retort be
carefully shut, which it is very difficult to do, there is a
continual current of air circulating round and escaping, and
the retort is very unequally as well as feebly heated.

A lamp furnace, and lamp, have been sometimes used for
distillations : and they are, no doubt, extremely neat and
convenient, when a very moderate heat is sufficient. But
spirit of wine is the only fuel we can employ. To manage
an oil flame requires more attention than a fire of charcoal or
coaks.

Some have proposed a train of furnaces, or sand heats, for
the hot air, or heat of an athanor furnace, to pass through,
but these are of little use.

A variety of other furnaces are used in various chemical
arts, as enamelling, smelting metals from their ores, and re-
fining and separating them from one another. But a des-
cription of those furnaces will not be instructive, unless in
giving a particular account of those arts themselves, which
we cannot now do. I have only given you the chief varieties
commonly used by speculative chemists in the way of inquiry
and experiment, explaining, as I went on, their principle and
mode of action. The same principles affect the performance
of all furnaces, and, therefore, this knowledge of them not only
enables you to construct such furnaces as I have described,
but also to construct furnaces properly accommodated to the
circumstances which occur in the practice of the chemical

arts. I described the glass-house furnace, because the great dome is a part which does not occur in the furnaces of the philosophical chemist : and yet an intelligent chemist may find many cases where it may be applied to other furnaces employed in a chemical manufacture. It has been success-fully employed to increase the heat in small furnaces for smelting iron from the ore, and in very small lime-kilns.

But beside the contrivance of these *particular* furnaces, as they may be called, attempts have been made to contrive what may be called a GENERAL FURNACE, that is, one applicable to all the chemical operations. The attempts of this kind which have been attended with any remarkable degree of success, are not many in number.

The first, so far as I know, which drew any attention, was that of Becher, the famous German chemist. He published a pamphlet, to which he gives the mysterious title of *Tripos Hermeticus Fatidicus* ; the Chemical Fortune-telling Tripod ; by means of which, to wit, an adventurer in alchymy was to learn, by trial of his own skill, whether he should be success-ful or not. The pamphlet contains the description and draw-ings of a furnace, consisting of four or five different pieces, which can be joined variously together, so as to adapt the furnace more or less completely to the purposes of digestion, or fusion, or the essaying and refinement of metals, or dis-tillation, either with earthen retorts and a strong heat, or the common still, or in glass vessels set in warm sand, or in hot water. You may see the drawings and description of this furnace at the end of Dr. Shaw's English translation of Boer-haave's chemistry, Dr. Shaw having copied it from Becher without mentioning his name. The different parts of this furnace are made of earthen ware, or a sort of clay, such as fire-bricks are made of ; but our potters here are not accustom-ed to such work.

This furnace is constructed with much ingenuity and judgment, and does honour to this early chemist. When ac-curately made, so that the joinings of its different parts can be made tight with a little chalk, I believe that it may be easily managed in all its functions. But such earthen ware

is very subject to crack.   This may be secured by hooping.   But frequent placing and shifting the pieces on each other, soon chips the edges; the joinings grow open; and then there is no regularity in the heats, and veins of cold air get in and crack our vessels.   A pottery, also, which is sufficiently strong for the furnace, will transmit the heat so much as to become extremely disagreeable ; and it becomes very troublesome to keep receivers cool enough for condensing the vapours.

Another contrivance, by which we can more readily provide ourselves in this country with a general furnace, or a set of furnaces for chemical experiments, is described by Mr. Lewis in his *Commercium   hilosophico-Technicum*. The author of this contrivance was a goldsmith.   Being daily accustomed to use crucibles, it occurred to him that those made of black-lead, some of which are of a large size, might easily be converted into furnaces, which would serve for chemical experiments on small quantities of matter.   The black-lead crucibles have the advantage of being so soft, that it is easy to bore holes through them, to cut doors in the sides of them, or to cut or grind off any part of them to adapt them to such a purpose.   And they also bear unequal heat, or sudden changes, with far less danger of cracking or splitting than earthen ware does. Mr. Lewis, therefore, teaches how the largest of them may be converted into furnaces for different purposes, either singly in some cases, or by joining two of them together in others; and, in others, by additional tubes, or other pieces which he adapts to them.   As he gives exact descriptions and delineations of all this, I need not be more particular here, but refer you to his work.   The contrivance is certainly ingenious, and affords a short and ready method for providing small furnaces.   But they have their faults.   They are not sufficiently durable, black-lead being consumed or destroyed by the repeated action of violent heat.   Neither do they contain enough of fuel ; nor can they be regulated with sufficient-exactness.

I have therefore endeavoured to contrive a general furnace for myself, which might be free from those imperfections, and I shall now describe it.   It has the adv.

vantage of being far more simple than those I have already mentioned.

Plate-iron is by far the best material for the outside of an experimental furnace: but, as its metal communicates heat very fast, this must be cut off by a proper lute lining. I have so far succeeded in this respect, that my furnace, though only two inches thick in the middle, will not scorch paper applied to its outside, when it is melting iron within.   I make it of the simplest rectilineal shapes, because workmen find great difficulty in executing curved and uncommon forms; and not one of a score of them will do it with accuracy.   Indeed, those highly praised forms seem to me of very little importance in most cases. The only case in which I find it of value, is in the laboratory of a reverberatory furnace, where something may be done in this way for mixing the hot air and flame, and making it play with force on the subject to be treated.

The body or fire-place is the only part of this furnace that requires description; the ash-pit, with its door and registers and grate, being constructed as in any other furnace.   It will be easily understood by considering the section represented in fig. 10.

The base, represented by the dotted line ABC, and the top KLM, are oval plates of iron, the longer diameter AC, being to the shorter as three to two nearly.   The base and top are equal, so that the sides KA, MC, are upright, the whole body forming an oval cylinder.   DEF is half of the hole in the bottom, which is occupied by the grate fixed on the top of the ash-pit.   GHI is half of the mouth of the furnace, which receives a still, or a sand-pot for distillation, with a retort.   This is a little nearer to the front K of the top, than the grate hole is to the front A of the bottom, so that the luting is thicker below than above.   Near the back M of the furnace, is a smaller hole P for the vent.   The luting at Q and R is so formed, that the cavity of the furnace does not greatly differ from a cylinder, except in so far as the vent PO does not communicate with it abruptly, but is gradually curved downwards, as represented in the figure, making the

middle of the cavity more roomy backwards, by which means it contains a greater quantity of fuel. S is the section of the luting, which forms a sort of arch, or bridge, contracting the entry of the vent. An iron pipe is set on at P, to increase the draught of the chimney. The fuel is put into the furnace by the aperture P, and the sloping form of the cavity causes it to distribute itself pretty uniformly.

When the furnace is used for smelting, the crucible is set on a pedestal standing on the grate, and the fuel is placed round it with great ease, the mouth of the furnace being open. This is then shut up by a stopper made on purpose, or by a flat fire-tile simply laid on it.

When we would distil with a naked fire, the retort has its bulb resting on a ring which hangs on the mouth of the furnace by three hooks, and the neck of the retort lies over the front of the furnace. The space round the retort at the mouth of the furnace is closed, as much as is necessary, by two or three pieces of tile, shaped so as nearly to fit the bulb of the retort when they are laid on the mouth of the furnace. A quantity of light ashes are now to be laid on these tiles, and heaped up so as to cover the bulb and part of the neck of the retort. I find that this produces a very gradual diminution of the heat as it recedes from the fuel, and is less liable to crack the retort, by inequality of heat, than any contrivance I know. Scarcely any process occurs which this furnace does not answer with great ease.

We have now said enough on the subject of furnaces. There is only one article more of the apparatus which remains to be mentioned,....I mean the LUTES.

In using the furnaces which I have described as most convenient for experimental chemistry, (I mean those made of plate iron) it is necessary that the iron be defended from the heat by lining or lute, as we called it, on the inside; and such lutes are necessary on other occasions in chemistry; as when we have occasion to close the joining of vessels with one another, or to give a coating to retorts, or even to crucibles, which is sometimes done. The materials employed for these purposes have their general de-

nomination from clay, of which some of the most useful
are partly composed, though there are some that do not
contain any of it.   They may be divided into such as con-
tain animal or vegetable matter, of the glutinous or adhesive
kind, and such as are composed only of earthy substances.
The first are used for closing the joining of vessels, when
the heat we mean to apply is not to be strong, nor the va-
pours to be produced corrosive.   The second serve for
the lining of furnaces, or for closing the joinings of vessels,
in operations in which the vapours are very corrosive, or
in which a strong heat must be employed, which would
scorch, or burn and destroy any animal or vegetable glutin-
ous matter.

   The joinings of vessels with one another, which we
have the most frequent occasion to close up by means of
lutes, are those of retorts with receivers.   And we may
remark, in the first place, with regard to these, that there
are not many operations in which it is necessary to make
the joining perfectly close, except when the receiver is
provided with an air-pipe. ' On the contrary, it is danger-
ous, on account of the air which must be allowed to escape
in some manner.   Therefore we are not anxious to con-
trive the most close and compact.   They are sufficient,
and better, if they be moderately so, and, in some cases,
when we think the lute too close, we even obviate it by a
pin-hole.   The animal and vegetable lutes, employed in
this way, are glue and chalk mixed in thin paste, and spread
on slips of paper, or gum Arabic and chalk, used in the
same manner, or flour and water, or a bladder, or lintseed
meal, or fat lute. Mr. Lavoisier recommends, for joinings
which we desire to be air-tight, but which are not to be
exposed to heat, the following: to 16 ounces of bees-wax
add 1 ½ or 2 of turpentine, and keep it for use.   When
used, soften, and make it tough, by warming and working
between the fingers ; then put it on the joint in little rolls,
and make it close ; and, lastly, cover it with slips of wet
bladder laced with pack-thread.   But, if the joint is liable
to be warmed or heated during the operation, you must
take fat lute.   This is made of raw pipe clay and lintseed

oil, beaten together very hard, to the consistence of a stiff adhesive paste.

Of the second kind of lutes, called the Fire-Lutes, a great variety have been proposed, and some of them compositions of many ingredients, some of which I have tried, but find none equal or superior to clay and sand, viz. sand 3, or 4, or 5, or 6, to clay 1. These are for luting vessels together, and for coating. But in lining furnaces, I use a double lining; 1*st*, a charcoal lute; 2*dly*, a fire-lute.

It may appear paradoxical to speak of a charcoal lute for lining a furnace. . But I find that a layer of powdered charcoal, beaten up, or kneaded, with as little water as will give its particles adhesion enough to attach itself to the metal sides of the furnace, by means of cautious beating, forms a firm stratum, which is the most imperfect conductor of heat of all that I have tried. When this layer of charcoal is defended from the action of the air by a layer of fire-lute, composed of one part of fine clay, and three or four parts of sand, carefully put on, and consolidated by gently beating it from day to day, till it no longer receives an impression from the mallet, it will last as long as any part of the furnace. Its durability will be greatly improved, without much change in its conducting power, by using, instead of pure water, water made muddy by about 1-20th of pipe-clay. If finely powdered charcoal be kneaded with 1-5th of pipe-clay, it may be kneaded and formed into any shape, and will be so impervious to heat, that a bit of it may be held in the fingers, within an inch of where it is red hot. Such a composition is, therefore, very proper for the doors of furnaces, and for stopples for such apertures as must be frequently opened and shut.

Thus have I taken a survey of the apparatus, and have thrown in such remarks upon the manner of using several parts of it, as could be delivered in a general way. It is not necessary at present, and it would be tiresome, to enter into further details. You will have the opportunity of observing the conduct of a variety of operations which will be performed during the rest of this course; and such as desire to make this part of chemical knowledge an object of their attention, will find instructions for per-

forming every particular process in the numerous books upon the Practice of Chemistry....Macquer, Boerhaave, Lemery, Wilson, &c.

We have now gone over all the articles that can with propriety be called general, affecting every branch of chemical inquiry, or requiring to be well understood, in order that we may prosecute this inquiry with skill and success. Our next occupation will be to examine the chemical properties, and mutual relations, of that immense variety of bodies which nature presents to our view. The natural philosopher, or mechanician, looks at the same object that we do, but with very different purposes. He only inquires whether the body be moveable, and what force will move it ; if it will move altogether, when touched in any one part; or if it will separate like sand or water. He inquires whether it be hard, soft, liquid, or elastic ; and the interesting properties which he finds in one, he finds in all ; and they are not only general, but obvious. To the mechanician, a pound of gold, and a pound of free-stone, are nearly the same.

The natural historian sees much greater varieties in the objects around him ; but, if he strictly keeps within the pale of his science, he sees only the surface ; he attends to the colour, the shape, the configuration, the structure, and perhaps the strength of the connection of the different parts.

To the mechanician and the natural historian, gunpowder is little more than an assemblage of black grains, of a certain weight and firmness. What a different and interesting object is it to the chemist, when he examines it by his instruments, mixture and heat ! He can get aquafortis and potash out of it, and brimstone, which all the powers of mechanism cannot separate from it, and which the natural historian cannot discover in it. If the chemist apply his other great instrument of research to it, what wonderful effects and productions will he now behold ! A force appears, of which it gave not the least indication, and which exceeds all the powers of machinery. Scorching heat, dazzling light, and deafening noise, strike him at once ; his gunpowder is dissipated, not into its ingredients, for

neither aquafortis, potash, nor brimstone, are to be seen; and, instead of a cubic inch of gunpowder, he finds a vast volume of elastic air.

Thus do those two instruments of knowledge, those two ladders of science, give us access to the most remarkable, and the most interesting properties and relations of bodies. Every step we take gives us a new and useful acquaintance. If, therefore, you have found interest and instruction in what has hitherto occupied your attention in this place, I think that I can assure you of still more in the application of the knowledge you have acquired.

END OF THE FIRST VOLUME.

NOTES AND OBSERVATIONS

BY THE EDITOR.

[*Note* A. *p.* 58, *2d line.*]

THE reader will surely be sensible that the sources of error in the experiments for ascertaining this fundamental point are numerous, and are with difficulty avoided. We are not entitled to make a decisive inference from the experiments, unless they be numerous, and their unavoidable irregularities be as frequently on one side as on the other of the general inference. It was a natural expectation that equal increments of bulk should accompany equal increments of heat; yet we cannot say that this has a good foundation. Ignorant of the intimate nature of heat, we are ignorant of the manner and the force with which it is accumulated round the particles of a body. But, on the other hand, the observation of the variations of bulk afford us the best means of discovering those hidden circumstances. Philosophers acquiesced too readily in the opinion that the increments of bulk were proportional to those of heat. The experiments of Dr. Brook Taylor, made with great judgment, seemed to confirm this opinion. Those of Rhenaldini did not contradict it. Sir Isaac Newton's experiments, proceeding on this supposition, combined with a most ingenious supposition concerning the

progress of heating and cooling, exhibited a happy coinci-
dence of facts, depending on principles totally different, which
seemed to put this opinion beyond all doubt. Dr. Black's
experiments in 1760 perfectly coincided with it.

But Mr. De Luc, one of the most assiduous, and perhaps
most scrupulous philosopher of Europe, had more than doubts
about the certainty of this correspondence. He had noticed
the observation of Beaumé, that water not only increased
greatly its bulk in freezing, but that its contraction by cold
ceased *before* its conversion into ice, and that it even expand-
ed a little. He verified this, and ascertained its quantity by
careful experiments, of a very simple and unexceptionable
kind, namely, by comparing the contractions of a quicksil-
ver and a water thermometer. This being the case, there
would be great uncertainty in the indications of temperature
by a water thermometer, or one containing any water in its
composition, seeing that there must be a condition of greatest
density; and therefore, on each side of the corresponding
temperature, the water will have the same bulk in very dif-
ferent temperatures. The extent of this uncertainty is un-
known. The curious experiments of Captain Williams at
Quebec, in which he froze water in bomb-shells, shew that
before it congeals, it expands prodigiously and *gradually*.
This must be inferred from the expulsion of the iron plug,
and protrusion of a long cylinder of ice, in cases when the
shell did not burst. It were worth examination what would
be the progress of its expansion, while cooling undisturbed,
below the ordinary freezing temperature. But are we certain
that during this expansion of water in temperatures below 38°,
the water is giving out heat in the same manner as during its
contraction, in cooling from a state of higher temperature?
Mr. De Luc now saw the necessity of following out this
comparison through the whole range of the thermometer.
For, since there is a temperature a little above that of melting
ice, in which a change of heat makes no change in the bulk of
water, it necessarily follows that equal variations of tempera-
ture on both sides of this degree of heat, will be accompanied
by variations of bulk, which continually increase. Accord

ingly, he found that while the mercurial thermometer increas-
ed its bulk equably, the increments of the bulk of the water
continually increased.   But, farther, when he endeavoured to
determine the proportion of this augmentation of the succes-
sive increments of the water, corresponding to equal incre-
ments of the mercury, he found that when the temperature
approached to that of boiling water, these augmentations of
bulk increased with increasing rapidity, and were remarkably
irregular.   He found the same thing in spirit of wine and
other fluids.   Some fluids, however, such as mercury, olive
oil, lintseed oil, oil of camomile, and others, contract in freez-
ing, and are not subject to all this irregularity.   In such fluids,
the acceleration of their expansion by heat begins at the very
departure from the state of solidity.   Hence Mr. De Luc
concludes that their expansion is more corresponding to the
changes of heat, in proportion as they are farther from either
the freezing or the boiling temperatures.

   Mr. De Luc's reasoning on this subject is in some degree
hypothetical, depending on his notions of the relation of bodies
to heat.   But he has established beyond dispute the fact, that
the successive augmentation of bulk by equal increments of
heat are continually increasing.   Mr. De Luc makes one in-
ference which cannot be controverted, and is of importance to
the philosopher ; namely, that is the best fluid for a thermom-
eter, which has the greatest interval between its freezing and
boiling temperatures ; because the successive augmentation
of bulk towards the middle of its range will the most nearly
correspond with the augmentations of heat.   Were it not that
there is now no difference of opinion, this circumstance alone
would justify the preference which De Luc gives to mercury
above all other fluids.   Its deviations from equable expansion
by equal changes of temperature, in the cases which most fre-
quently occur in our experiments, are so trifling, and indeed
so uncertain, that they need scarcely be attended to.   They
are called uncertain, because it is plain that the nature of the
experiments for ascertaining the real medium temperature,
arising from the mixture of a hotter and a colder fluid, seems

unsusceptible of the accuracy necessary for determining such small quantities with precision.

The annexed table of Mr. De Luc's comparison of thermometers filled with different fluids cannot but be acceptable.

He had determined the expansion of mercury by very careful experiments; and found that in the middle temperature between those of freezing and boiling water, that is, at 40° of Reaumur's thermometer, the mercurial thermometer stood at 38,6; and the others, whose scales were all of equal parts, containing 80 between these two temperatures, stood as follows:

Mercury,	38,6	15 : 14
Olive and lintseed oil,	37,8	15 : 13,4
Oil of camomile,	37,2	15 : 13
Water saturated with salt,	34,9	15 : 11,6
Rectified spirits of wine,	33,7	15 : 10,9
Water,	19,2	15 : 4,7

The second column expresses the proportion of the contraction from the temperature of boiling water, to the middle temperature 40°, to the contraction from 40° to 00°.

[*Note* B. *p.* 60, *3d line from bottom.*]

This method of measuring intense heats is extremely refined, and could occur only to such a mind as Newton's. It is, however, in some respects hypothetical and gratuitous, because it proceeds on a supposition which I apprehend cannot be demonstrated, namely, that the momentary emanations of heat, whether in the way of communication by contact, or by simple emission, or in both ways, are proportional to the excess of the temperature of the cooling body over that of the surrounding or receiving bodies. We know nothing of the connection of bodies with heat, from which this law of variation can be properly inferred. But, on the other hand, when we observe the progress of heating and cooling, in cases where we have other evidences of the quantities gained or lost, we find it wonderfully correspondent to this law; so much so in-

deed, that we cannot perceive any deviation from it which may not be accounted for by the circumstances of the case. This method, therefore, is a very valuable acquisition to the philo-sopher.

[*Note* 1. *p.* 81.]

It is somewhat strange, that neither Boerhaave, who ex-presses great curiosity about all the relations of bodies to heat, nor the great number of ingenious men who were much occu-pied with the studies connected with those relations, were in-duced, by the original experiment of Fahrenheit on a mixture of water and mercury, to examine so important a question as that concerning the proportion of heat in different substances having the same temperature. The experiment lay unnoticed, or was copied out by many compilers, without taking the trou-ble of thinking about it. Dr. Martin, also, though engaged in experiments which were very closely allied to this question, did not attend to it. The philosophers on the Continent seem agreed in giving the honours of the discovery of the unequal distribution of heat among bodies of different kinds, to Professor Wilcke of Stockholm, who read a dissertation on it in the Royal Academy in 1771. (*Swed. Trans. vol.* 35. *p.* 93. *New Swed. Trans.* 1782, *Part Second.*)

But Dr. Black had seen its importance long before, and had discovered many cases of very unequal distribution. In some small note books, written while a student of medicine in Edin-burgh, he sets down queries, founded on Fahrenheit's expe-riment, and projects of experiments, on a variety of substances which had exhibited puzzling appearances in this respect. He had made several of these before 1757, and, among these, some which must have given rise, in his thoughts, to his celebrated theories of fluidity and vapour.

Mr. Watt of Birmingham was, as far as I can learn, the first who considered this subject steadily, and in a system. He was strongly incited to this examination by the great object which then occupied his attention,....the improvement of the

steam engine.   From Dr. Black's lectures he had learned the doctrine of latent heat, and saw that a great obstacle to the performance of that noble engine was the waste of steam by ineffective condensation.   It was, therefore, an interesting object to him, during his first speculations, to learn the quantity thus wasted by warming the cylinder and other parts of the apparatus ; and some of his earliest attempts to improve the engine consisted in employing cylinders made of substances which required little heat for warming them.   He made many experiments on various substances, in order to learn their properties in this respect.   Though rude, they were sufficient for his purpose ; and the examination would have been carried much farther, had he not contrived the separate condenser, which made this property of bodies of little moment to him.

Mr. Watt's experiments were all communicated to his preceptor, who took the warmest interest in them ; and this correspondence gave him the opportunity of knowing the uncommon genius and the great worth of his pupil, and was the beginning of a friendship which lasted through life, and which, for the merit of the parties, and for mutual confidence, respect, and attachment, has had few equals.

Before the year 1765, Dr. Black had made many experiments on the heats communicated to water by different solid bodies, and had completely established their regular and steady differences in this respect.   He mentioned these in his lectures in the University of Glasgow, and was occupied with the experiments for the same purposes when he was called to Edinburgh.   He was much assisted in the experiments by Mr. Irvine, who afterwards lectured from the same chair.   The registers of these experiments remain among Dr. Black's papers, most of them dated, and partly written by himself, and partly by Mr. Irvine.   They are all previous to 1770.   Mr. Irvine made many experiments on the same subject, besides those in which he assisted Dr. Black ; and particularly on those fluids, which, without any remarkable chemical action, produce heat by mixture ; such as water and vitriolic acid, water and spirits of wine, &c.   And it was this train of experiments which led Dr. Irvine to that ingenious and plausible

method of determining the point of absolute privation of heat, which he publicly taught in his lectures, and communicated to his philosophical friends. His fundamental proposition was, that " the heat which appeared in mixing vitriolic acid and wa- " ter is the difference between the sum of the absolute heats of " the two ingredients, and the absolute heat of the mixture, " while the heats, which each of them separately required for " an equal variation of temperature, had the proportion of their " respective absolute heats." Therefore, having discovered, by such experiments, the difference, and the ratio of the abso- lute heats of the ingredients, we can find those absolute heats, and the temperature at which those heats commence, or in which the ingredients contain no heat at all.

As Dr. Black and Dr. Irvine now lived at a distance from each other, and each was assiduously occupied with his official duties, and interested in acquiring a deserved reputation, their mutual communications were less frequent than formerly, but they were by no means given over. Dr. Black's speculations were anxiously directed to one favourite object,....the theory of fluidity and vapour; and Dr. Irvine's to the discovery of the absolute heats of bodies. It was in the prosecution of this investigation that Dr. Irvine found himself obliged to use some appropriate terms for expressing his peculiar views of the sub- ject. That quality by which some substances required more heat than others to produce the same elevation of temperature, had different names at different seasons during his progress. *Affinity* pleased him a long while, and was thus employed both by Dr. Black and Dr. Irvine. Both also used the terms *capa- city, faculty for receiving heat, appetite for heat*, &c. &c. *Capacity* was at last adopted, as the best suited to all the oc- casional modifications of this property which appeared in the experiments. It was the familiar language among the stu- dents of chemistry, both in Glasgow and Edinburgh; and I have not the least doubt but that the Swedish gentlemen, of whom there were several at Glasgow and Edinburgh between 1763 and 1770, carried home with them both the doctrines and language which were familiar to them. These facts, which are all consistent with my personal knowledge, having

been myself on the spot, are of some use for ascertaining the value of many claims of originality and priority which have been too warmly disputed. The great merits of a Wilcke, a Bergmann, a Lambert, are not diminished, although we learn that Dr. Black and Dr. Irvine had preceded them in some of their ingenious researches.

Before concluding this note, it will not be unacceptable, if the general view of this doctrine of the *specific capacities* of bodies for heat be communicated in a compendious manner, and the method of applying the principle to the chief questions, which may be proposed, be briefly described.

It is assumed, in the first place, that the proportion of the quantities of heat required for changing the temperature of the two bodies equally, is constant. This is not unlikely ; but it is by no means demonstrated : Nay, the experiments of Dr. Black and Dr. Irvine on bees wax, spermaceti, &c. directly prove the contrary ; in all cases at least where the body becomes gradually softer and softer before melting. We are not certain but that something of this may obtain in most substances. Lichtenberg thinks that water itself becomes more perfectly fluid by increasing its heat. But the change observed by him was so trifling as not to cause any sensible variation in its comparative capacity, in all the temperatures between freezing and boiling.

Dr. Black estimated the capacities, by mixing the two bodies in equal masses, but of different temperatures ; and then stated *their capacities as inversely proportional to the changes of temperature of each by the mixture.* Thus, a pound of gold, of the temperature 150°, being suddenly mixed with a pound of water, of the temperature 50°, raises it to 55° nearly : Therefore the capacity of gold is to that of an equal weight of water as 5 to 95, or as 1 to 19 ; for the gold loses 95°, and the water gains 5°.

It will be most convenient to compare all bodies with water, and to express the capacity of water by unity, or to call it 1. Let the quantity of the water be W, and its temperature w. Let the quantity of the other body be B, and its temperature b. Let the temperature of the mixture be m. The capacity of

B is $\dfrac{W \times \overline{m - w}}{B \times \overline{b - m}}$ Or, when the water has been the hotter of

the two, the capacity of B is $\dfrac{W \times \overline{w - m}}{B \times \overline{m - b}}$ In other words,

multiply the weight of the water by its change of temperature.
Do the same for the other substance. Divide the first pro-
duct by the second. The quotient is the capacity of the other
substance, that of water being accounted 1.

These experiments require the most scrupulous attention
to many circumstances which may affect the result. 1. The
mixture must be made in a very extended surface, that it
may quickly attain the medium temperature. 2. The stuff
which is poured into the other should have the tempera-
ture of the room, that no change may happen in pouring it
out of its containing vessel. 3. The effect of the vessel
in which the mixture is made must be considered. 4. Less
chance of error will be incurred when the substances are of
equal bulk. 5. The change of temperature of the mixture,
during a few successive moments, must be observed, in
order to obtain the real temperature at the beginning.
6. No substances should be mixed which produce any
change of temperature by their chemical action, or which
change their temperature, if mixed when of the same
temperature. 7. Each substance must be compared in a
variety of temperatures, lest the ratio of the capacities
should be different in different temperatures.

When all these circumstances have been duly attended
to, we obtain the measure of the *capacities* of different
substances for heat.

Philosophers do not seem agreed as to the meaning
affixed to this term. It is frequently used in the sense of
mere *capaciousness*, or *room* for the admission of heat.
But this is a very needless consideration, and unconnected
with all chemical science. Heat is not merely *admitted*
into bodies, but *absorbed* or *drawn* into them, in conse-
quence of some tendency to combination, whatever the
nature of this union may be ; whether mere adhesion, like
capillary attraction, or the mixture of metals or of watery
fluids ; or like the solution of saline crystals in water, and
the mixture of brines with more water ; or like the union

of salt and water in crystallization ; or like the union of acids and alkalis. In all these examples, there is some physical action, and this is continued till an equilibrium of the acting forces is effected. This is very different from mere capaciousness, and it even receives no explanation by the help of capaciousness.

The only thing expressed by the numbers above mentioned is the *variability of temperature by heat*.

Dr. Crawford, in his essay on animal heat, has given tables of the capacities of a great number of substances. But a want of attention to some of the above-mentioned cautions, particularly the sixth *, has led him into many mistakes ; and therefore, in a second edition he has made very great changes in his measures. But even his second measures seem exceedingly gratuitous and uncertain. Mr. Kirwan has also ascertained the capacities of a considerable number of bodies. His table is published in *Magellan's dissertations*. Bergmann has given another table in his *essays on elective attraction*. Baader has given another in his *Dissertation on heat;* and, lastly, Gadolin has given a large list, in his *Theory of the specific Heat of Bodies, Abo.* 1784.

Wilcke calls the number arising from *our* formula, the measure of the *specific heats*. But if the quantities of the substances are reckoned by their bulk, then the resulting numbers are the measures of what he calls the *relative heats*. The relative heats may be obtained by multiplying the specific heats by the specific gravity of the substance compared with water in the experiment. Thus, the relative heat of gold is 1, or equal to that of water ; while its specific is only 0,0526, or $\frac{1}{19}$. The comparison is most frequently made by measures in bulk, in which case the formula gives the relative heats. But Wilcke thinks the measures by weight more proper, because the question seems to relate chiefly to the peculiar nature of the matter itself, independent of all consideration of space.

* Dr. Crawford was particularly interested in the specific heats of the substances used by animals for food. Most of these generate heat or cold, when mixed with water. Flour of all kinds does this in a remarkable degree. Even different kinds of flour generate heat, by mixing them suddenly together in great quantities.

Dr. Irvine, Dr. Crawford, and most of the other writers on this subject, have considered those numbers as expressive of the proportions of the absolute quantities of heat, or cause of heat, contained in bodies, when they are all of one temperature.   Indeed, it is this opinion which gives the chief importance to the whole doctrine of specific heats. But this opinion is just, only on the supposition that the measures, obtained by the above described experiments and calculation, are constantly the same, whatever the temperatures may be in which the experiments are made. Dr. Irvine's ingenious method of discovering the temperature of absolute privation, evidently presupposes this constancy of specific heat; or, if they are not constant, it supposes that we know the whole law of variation.   Now both of these assumptions are highly improbable.   In none of the progressions of natural operations that we are acquainted with, do we find this constancy.   It is much more analogous to other phenomena, to suppose, that, in the temperatures near to that of absolute privation, the quantities of heat necessary for producing equal elevation gradually diminish, and this, perhaps, without end, like the distance of the hyperbola from its assymptote. It is equally probable that the law of diminution may be different in different substances.   This will cause the measures of specific heats to change their proportions continually; and therefore the specific capacities observed in temperatures, all of which are far removed from that of the entire absence of heat, give us no means of obtaining the proportions of the accumulated sum of all the heats which has been received into the substances.   It follows from this, that even although it should be granted to Dr. Irvine, that the heat which emerges, in mixing vitriolic acid and water, or in the freezing of water, is the difference between the absolute heat of the mixture, or the ice, and the absolute heats of the substances before mixture, or of the water before freezing, still we cannot ascertain those absolute heats, or the temperature of no heat.

Accordingly, it appears that it has been only in a very few cases that Dr. Irvine found a tolerable coincidence of his determination of this extreme cold: and the deter-

mination by means of mixtures, differed enormously from those obtained by means of congelation ; and still more from those obtained by means of the condensation of vapour.   The observations of Gadolin on these experiments are highly instructive and judicious.

Upon the whole, it appears that, although the informations which we have acquired by those experiments must be, on many occasions, very useful, yet they give us little assistance for determining either the proportions, or the real quantities of the matter or cause of heat contained in bodies, or the temperatures in which any body contains no heat at all.   It is not even certain that adjoining bodies will all acquire one temperature.   In short, we are as yet too little acquainted with the nature of heat, and the laws of its connection with other substances, to pronounce with confidence on any of these questions. We must be better informed concerning the mutual relation of the particles of this supposed substance, and concerning the powers or forces which combine it with other matter.   There must, in every quiescent or permanent condition of a body with respect to heat, be an equilibrium between all these forces and the forces which connect the particles of other matter.   This is a subject more of a mechanical than chemical nature.   The chemist will not perhaps derive much advantage from a perfect knowledge of the subject ; and he will contribute more to the advancement of his own science by overlooking this research, and attaching himself to the general though complex facts that occur in his experiments.   Establishing a general fact in chemistry is obtaining a fixed point, from which we can set out on any farther search.   In this respect, the tables of specific or relative heats, or of capacities, are real additions to chemical science, and will often assist us in the explanation of phenomena still more complicated, although we still remain ignorant of the principle that unites them all.   Newton established a noble body of astronomical science, although he professed total ignorance of the nature of gravity. Dr. Black, ever more desirous of communicating truly chemical knowledge than refined philosophical doctrines, has generally avoided all such disquisitions in his lectures.

These were intended for the information of even such as had no previous scientific education: and it was always with some reluctance that he engaged in abstruse specula-tions.

[*Note* 2, *p.* 83.]

It must be acknowledged, however, that this proportion is not supported by any very precise experiments, although such are neither abstruse nor difficult. There are even circumstances well known to us, which cannot but have a notable influence on the heating and cooling of bo-dies. We know that heat is much more speedily commu-nicated to the remoter parts in some bodies than in others, when the heating substance is applied to one extremity of the body. Dr. Black takes particular notice of this distinc-tion in a subsequent part of his lectures. This being the case, it must follow that bodies which conduct heat slowly along their own substance, must also imbibe and emit it slowly, because their superficial parts transmit slowly what they have received from the heating cause to the interior parts. The imbibition and emission of heat will also depend, for the same reason, on the conducting power of the heating or cooling medium.

But, besides this obvious cause of difference, there are circumstances of a more abstruse nature, which render the truth of this proposition very doubtful, and make it neces-sary to have it evinced by very unquestionable experiments. There are two ways in which heat is received or emitted by a body. First, by communication in contact with the body from which it comes, or into which it goes. The transmission arises from the want of that equilibrium of connecting forces, which produces a permanent condition of things. Now, since we find that different bodies con-tain different proportions of this moveable heat, it is plain that the equilibrium now mentioned must be broken in different degrees by the addition or abstraction of the same absolute quantity of heat; and therefore the quantity which will escape or come in, in equal times, so as to pro-

duce a new equilibrium, will be different,...that is, that heat will not be absorbed or emitted with equal celerity by all bodies.

In the next place, heat is unquestionably emitted from bodies also in the manner of light, or by what may be called *radiation*. This is performed with immense velocity, so as to seem instantaneously darted to any distance. A late writer indeed has asserted, in *Nicholson's Philosophical Journal for November* 1800, that heat is propagated only by communication through the air, and therefore slowly. But this is contrary to most distinct and varied experiment. The very curious observations of Dr. Herschell shew that heat is radiated in the same manner as light, *(Phil. Trans.* 1800,*)* and is refracted and reflected also in a similar manner. It may here be observed, by the way, that this observation of the constant ratio of the sines of incidence and refraction, is the strongest argument that has yet been obtained for the materiality of heat. For this law of motion is competent only to a material particle, having mobility and inertia, and acted on by transverse accelerating forces, in the same manner that a cannon ball is acted on by gravity, and by the force of projection. Mr. Scheele is considered as the first person who observed and established this radiation of heat, and its separability from light. But it was observed much more than a century ago by Mr. Robert Hooke, and exhibited by him to the Royal Society at the meeting of May 15, 1682. *(Birche's History*, IV. 137.) Mr. Pictet, of Geneva, also has made some remarkable observations and experiments, which shew that heat is continually emitted from all bodies warmer than the medium, although by very small degrees. This is confirmed by the direct observations of Dr. Herschell.

Now in this case, as we are entirely ignorant of the forces which expel this heat from the body, and of their laws of action, and of the proportion of the heat radiated to that carried off by contact, and whether this proportion be constant or variable, it seems unwarranted to assume this proposition as true, without any direct proof from observation. We have no such proof, nor indeed any observation of the

comparative celerity from different bodies, sufficiently pre-
cise to give us distinct knowledge either of its truth or
falsehood.

[*Note* 3, *p.* 96.]

We must not omit taking notice of a very singular dis-
tinction, which Count Rumford has endeavoured to esta-
blish between the conducting power of solid and fluid
bodies, namely, that while heat is propagated along solid
bodies, from particle to particle, according to certain regu-
lar laws, fluid bodies are heated in their remote parts only
by the actual transference of the particle which received
heat from the hot body when in contact; and that, without
this motion of the heated particles, occasioned by their ex-
pansion, and consequent levity, the remote parts of a fluid
would never be warmed.   The ingenious and worthy
author has supported this opinion by a series of curious
and well contrived experiments, the results of which are
most unexpected and surprising.   They are to be seen in
the 7th of his essays.

Dr. Black was much struck with the novelty and im-
portance of this opinion, and began to examine it with that
cautious exactness which he had always practised; but the
great decline of his health and strength soon checked his
progress, and the few notes on the subject which are to be
found among his papers, are insufficient for giving any no-
tion of his sentiments.

It is clearly established by the experiments of Count
Rumford, that when the internal motion, producible by the
inequality of specific gravity, occasioned by heating the
lower parts of a fluid, is prevented, the progress of heating
is almost insensible, and is no more than what we may ex-
pect by means of the solid matter of the vessel, and by
radiation.   It is also certain, that our knowledge of the
relation or connection of a particle of matter to heat, gives
us no information of the manner how heat, attached to one
particle, is shared with or communicated to another.   It
is the perfect equality of action in all directions, which fits

a particle of matter for being the particle of a fluid; while a certain polarity, arising from an inequality of action in different directions, would fit it for being the particle of a solid. It is not at all unlikely that this difference may so change this attraction (or whatever name is given to the relation) of a particle for heat, that a superfluity of heat may have a tendency to quit the *pole* of one particle for the *pole* of another, in a solid body, while there is no such tendency with respect to a particle of a fluid, having no polarity. Heat may continue to accumulate spherically round the latter, expanding the molecula consisting of such particles, without leaving it for another. We see a difference of relation not less distinct and remarkable, in the case of liquids and gases. Liquids, like solids, expand in arithmetical progression, acquiring equal additions of bulk by equal additions of heat; whereas gases, such as common air, carbonic acid, &c. expand in geometrical progression by equal elevation of temperature. An increase of 30° nearly doubles the bulk of ordinary steam; another 30° doubles this bulk; or 60° quadruples the original bulk nearly. It may be added, that the phenomena of galvanism shew us still another modification of the chemical relations of atoms, by which a quality or power of producing effects is propagated to any distance, without our being able to conceive that any material substance is transported along this line of visible activity.

There is nothing, therefore, in the opinion offered with great caution by Count Rumford, that is incompatible or inconsistent with our most distinct notions on these subjects. It is a fair field of inquiry, most ingeniously laid open to philosophers. It even promises to assist us in acquiring juster notions of chemical action, than those which at present satisfy chemists: and they would do well to study it mathematically, as P. Boscovich has done. This would soon make them exclude some modes of action which are in fashion, and by thus limiting the attention, would sooner bring us into the right path.

[*Note* 4. *p.* 138.]

The celebrated Bergmann having learned and adopted Dr. Black's theory of fluidity, imagined that this absorption of heat in a latent state would afford an excellent method of examining the question so much debated by philosophers, " Whether heat be ponderous or not ?" The experiments which had been made for this purpose have been very anomalous, by reason of many unavoidable sources of inaccuracy, in all cases where great quantities of sensible heat are accumulated in bodies, in order to make its weight perceptible. Currents of air are produced, which affect the scales of the balances employed. The great heats expand the arms of the balance, which destroys the equilibrium, and affects the measurement. The very expansion of the body affects it, by changing its specific gravity.

Bergmann proposed to weigh, not the sensible heat, but the latent heat, which is accumulated in considerable quantities, in such a manner as to be free from errors. He proposed to weigh a mass of water, of the temperature 32° or 33°, in a room having this temperature, and then to freeze it completely ; and, having again brought the ice to the temperature 32°, to weigh it again in a room having that heat. Thus all the causes of error seem to be avoided.

This judicious experiment has been made with the most scrupulous care by Dr. Fordyce, *(See Phil. Trans.* 1785.*)* with about three and a half ounces of water, contained in a thin glass globe hermetically sealed. Its weight was examined by a balance which was sensible to the $\frac{1}{1000}$th part of a grain, and it was *repeatedly* found $\frac{1}{16}$th of a grain, nearly $\frac{1}{25600}$th of the whole, lighter when in the fluid state. Similar experiments have been made, having the same result, by Sir Benjamin Thomson, (now Count Rumford) by Mr. Gouvenain at Paris, and by others. Dr. Fordyce also found that the ice became $\frac{1}{16}$th of a grain heavier, when cooled down to 22°. This might be expected from the contraction of bulk, and from the downward current of air which so cold a mass would occasion.

Dr. Black seems disposed to ascribe this unexpected effect to a repulsion for the heat in the earth. This, however, considered as the weak exertion of the repellency which he supposes to be a property of the atoms of heat, is not consistent with such laws of relation, whether attraction or repulsion, as are necessary for the corpuscular combinations of matter. These must decrease at least in the inverse triplicate ratio of the distances, and be altogether insensible at any measurable distance. But it is much more reasonable to ascribe the small diminution observed, the 16th part of the 17000th part of the whole, to errors unavoidable in such experiments. Accordingly, Count Rumford, some years after, repeating the experiments on much larger quantities of materials, could observe no difference whatever. Nor should we expect to find heat ponderous. When we recollect that it comes with the same velocity as light from the sun, and yet exerts no sensible impulsive force, we have no reason to suppose the quantity of matter to have a sensible weight.

[*Note* 5. *p.* 189.]

The worthy author of these lectures was always more anxious to communicate what may be called a clear and confident knowledge of the doctrines of pure chemistry, than to lead his pupils into abstruse or refined speculations on the unseen and unknown immediate causes of chemical combination. He considered every question of this kind as rather out of the pale of chemical science; and so it certainly is. Whenever we speculate about the attractions and repulsions of particles, as the immediate agents in effecting the chemical changes, we are no longer chemists, but mechanicians. We are considering questions about local motion, and the mathematical determinations of the effects of moving forces. Not only is the occupation not chemical, but the questions themselves give little addition of chemical knowledge. This, at least in the present state of the science, is promoted chiefly, if not solely, by a more successful generalization of chemical phenomena.

This generalization is, in fact, the establishing of general laws, and marking with greater accuracy their distinctions and limitations. But the chemical phenomena which are to be classed are all of a very complicated kind; and it is of infinitely more consequence to know the full extent, and all the modifications of the law, than to know the hidden process of nature in any one of the phenomena; just as the noble system of physical astronomy, which Newton was able to class under one general fact (universal gravitation in the inverse duplicate ratio of the distance) was of more importance than the explanation how this fact was effected.

In all cases, therefore, Dr. Black contented himself with presenting his subject in the simplest form, suggesting the simplest notion of it. Nothing could be more simple than his doctrine of latent heat. The experience of more than a century had made us consider the thermometer as a sure and an accurate indicator of heat, and of all its variations. We had learned to distrust all others. Yet, in the liquefaction and vaporization of bodies, we had proofs uncontrovertible of the entrance of heat into the bodies. And we could, by suitable processes, get it out of them again. Dr. Black said that it was concealed in them,....*latet*,....it was as much concealed as carbonic acid is in marble, or water in zeolite,....it was concealed till Dr. Black detected it. He called it LATENT HEAT. He did not mean by this term that it was a different kind of heat from the heat which expanded bodies, but merely that it was concealed from our sense of heat, and from the thermometer.

It has always surprised me that a doctrine so plain, simple, and unassuming, should be misunderstood. For it must be misunderstood to be contradicted in any shape. Dr. Black would not have thought any more about a subject already so plain, nor would he have paid any attention to the condition or state in which this heat exists in the body, had he not been called upon by those who said that he had introduced an unnecessary principle into the doctrine of heat, and had he not heard it asserted that the heat was no more latent than while it expands a body. He knew this ; but asserted that the indication of its presence,

though undisputable, was not heating, or expanding, but melting a body, or converting it into vapour. It was only in order to answer this challenge that he was led to consider its peculiar condition, by which its mode of action was rendered so different from what it was before. His first observation was, that it was no longer so free and move-able as before, and that it was only the *additions* made to the heat necessary for mere fluidity which were now move-able, and affected the thermometer. He now saw good chemical reasons for considering this concealed heat as *united* with the substance of the body, in a way very much . resembling many chemical combinations. A warm body carried its warmth about with it. It shared its warmth with other bodies, just as a brine shares its salt with more water. In liquefaction and vaporization it attached to itself a determined quantity, and no more than the proper quantity, just as water does of salt, or as salt does of water in forming a crystal. The appearance, and many proper-ties and modes of activity, are changed by this union, just as in other chemical combinations. He was therefore led to consider heat as chemically combined, in all its modes of existence, whether in expanding, in melting, or in boiling off a fluid. He considered heat in a certain pro-portion as an *ingredient* of all liquids, and, in another pro-portion, as an ingredient in all vapours.

But all this was refused, and the combination of heat was denied, merely because the touch of a cold body could take out this heat,....because the combination is not perma-nent, like the generality of chemical combinations ; and it was asserted that this great quantity of heat was merely *received* into the body, and lodged there, without affecting the thermometer, merely because the body, by its change of form, had more room for it.

Dr. Black now saw strong reasons for adhering still more to the conception he had now formed of its mode of existence in the fluid and vapour. He now considered heat as the *active cause* both of fluidity and of vapour, producing those new modes of aggregation by a true chemical com-bination with the particles of the body. To the reasons adduced by Dr. Black for this opinion I would add one or

two more, which I think are incompatible with the notion of mere capacity.

When a piece of dry sponge lies in the exhausted receiver of an air-pump, and we admit the air into the receiver, it soon fills all the pores of the sponge. This has capacity for its reception, in the purest meaning of the term. The sponge exhibits no appearance of change upon this reception of air into its pores. But, let us pour water into the receiver, the sponge has the same capacity or room for the water. But how different the appearance! The sponge swells to twice its former bulk. And, although we understand very little of the corpuscular actions of bodies, we comprehend a good deal of this process. We know that the sponge does not merely *admit* the water into its pores ; it *attracts* it, and will even raise the water to a considerable height, if one end only of the sponge be dipped into the water. We even comprehend a little of the manner in which this attraction produces the expansion of the sponge. There is a force, which is mutual between the sponge and the water, acting at the mouth of the pore, urging the water into it, pressing it on the water already in, in the same manner as if a little piston were pressing on the water in the mouth of the pore, and therefore distending the pore. We can even calculate the limit to the effect of this force, and what rigidity of the pore will put a stop to the accumulation. We know that this attraction will keep the bodies united. And we say, with the utmost propriety, that they are united....combined....and we call this particular mode of union *adhesion*. And lastly, we say that the water is the cause of this distension of the sponge. It would be very awkward to say that the capacity of the sponge is the cause of this distension. It is very true that the cause of it is not the water, but the force which unites the water and the sponge. But the water is the occasion or cause, in the same manner that fixed alkali is the cause why lime is separated from its solution in an acid. If the experiment had been made with a piece of dry wood in the place of sponge, we know that it would have swelled with a force sufficient for splitting the firmest rocks. It would surely be ridiculous to ascribe this to the capacity

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of the wood.   If any person, unaccustomed to discussions
of this sort, finds a difficulty in conceiving how attraction,
which rather seems fitted for contracting the sponge, pro-
duces its expansion, he may see this effect of undoubted
attraction exerted between the particles of mercury. Lay
two or three small globules of mercury on a smooth plate
of glass : There can be no doubt of the mutual attraction
of the atoms being the cause of the round form of the
drops.   Lay another light plate of glass on these drops of
mercury: Its weight will flatten them a little.   Lay on
some small weights : This will flatten them still more.
The addition of more weight will increase this effect.
Now lift off the weights one after another.   The drops
will gradually contract their breadth again, *by raising the
upper plate farther from the under one.*   Here it is plain
that the mutual attraction of the particles forces the plates
asunder.

This reasoning is applicable, in the strictest manner,
to the phenomena now under consideration ; and it
is even more forcible in the case of some of them,....in
vaporization, for example.   Here we know that a due
supply of heat will cause an ounce of water to burst a
bomb-shell.   Can any person ascribe this to the *capacity*
of the steam?   This capacity is not yet acquired, nor *will*
it be again acquired, unless the due supply of heat be at
hand.   The particles of water acquire the vaporous ar-
rangement which renders them so capacious, *only in conse-
quence of the mutual action between them and the matter of
heat.*   In short, it is plain that the absorption of heat is
not the *consequence*, but the *cause* of the enlarged capacity.
Strictly speaking, they are concomitant events, and it is
the *combining force* that is the immediate and operative
cause of both.

Those who assign the enlargement of capacity as the
cause of the absorption of heat, in a state which does not
affect the thermometer, are by no means precise and uni-
form in the meaning which they affix to the term.   They
must be sensible that mere *capaciousness*, or *room*, will not
apply to the phenomenon of melting ice, in which the room
is undoubtedly diminished in the liquefaction, and even for

some time after. For, while the water, just formed, continues to accumulate heat around its particles, the bulk does not increase; on the contrary, it diminishes. In their conception of capacity is included the notion of a *disposition* to absorb heat; that is, of an *attraction* for heat; or, to express it more philosophically, the notion of the existence of a force, which unites the matter of heat with that of the ice. What is this but the union or combination which Dr. Black at last contended for? Chemistry presents a considerable variety of such combinations, and there are many, especially of the gases, which are as little permanent, and yield as readily to a difference in the proportion of the ingredients, as those combinations of bodies with heat. Nay, we shall find still another combination of heat with other matter, which has all the permanency that can be desired, and which the most pertinacious partisan of capacity must admit as a true chemical union. I mean the gaseous form. Oxygen gas is supposed to consist of the basis of vital air, united with the substances of heat and of light. It requires a certain temperature, and the presence of a proper third body, to produce a decomposition; and here we find a prodigious quantity of heat accumulated. Capacity will explain *this* accumulation, in the very same manner that it explains all the others.

I am disposed to think that there is a gradation of union between the particles of matter and heat, which very much resembles the gradation which late observations have discovered in the combinations of oxygen. The degrees of oxydation, as it is called, are as progressive, and as distinct, as those exhibited in the effects of heat; and they are all produced by the gradual introduction or abstraction of oxygen. There seems to be, first of all, a union similar to that of mere adhesion. The heat thus united has been called free, moveable heat. Perhaps the union accompanied by expansion, or which produces expansion, is something more close, and it may be that only the superfluity is free and moveable. This mode of union is evidently independent of the others, and may be superadded to them all. Then comes the union which is characterised by fluidity. This is followed by the union of vaporization,

and this by the union which forms a gas.   Perhaps a just
conception of these changes would be more easily sugges-
ted, were we always to employ the word *temperature* for the
degree of the scale, and *heat* for the matter combined; and
to call the heat concealed or latent, in a liquid, and in va-
pour, the *melting heat*, and the *boiling heat;* thus distin-
guishing them from the *melting temperature*, and *boiling
temperature.*

In short, this dispute seems merely occupied about the
propriety of a term: And even in this humble sense, Dr.
Black's employment of the term combination is more pro-
per than the employment actually made of the term capa-
city.   About the propriety of the term *latent heat*, as the
expression of a mere fact, there can be no dispute.

In Dr. Black's examination of Dr. Cleghorn's method
of explaining the phenomena of fluidity and vapour, he was
obliged to consider the uniting forces, not as a chemist,
but as a mechanician.   He disliked this occupation ex-
ceedingly; and the remarks on Dr. Cleghorn's hypothesis
are almost the only examples to be found in the manuscript
from which he lectured.   But observing this way of con-
sidering chemical subjects to be gaining ground, after
having been so long and so profitably banished from che-
mistry, he was induced to say more on the subject, in some
particular courses.   Some notes of those occasional lectures
remain.   I recollect a conversation, in which he explained
to me some of his notions of this kind.   He said that this
way of considering the phenomena was only mispending of
time, because it produced no clear conception, and gave
no confident knowledge.   He said that he sometimes
thought himself successful; but some other views con-
vinced him that he had only been surrounding, in imagina-
tion, the particles with material, active atmospheres, which,
notwithstanding their activity, and the mutual repellency
of their particles, must be conceived to penetrate each
other, without derangement of their forms; in short, with-
out the very activity which he had assigned them.   He
could recollect his being affected in the same way by read-
ing Dr. Gowen Knight's ingenious essay on magnetism.
The first steps of the investigation seemed to give very

clear notions; but he found them afterwards involved in continual inconsistencies. He had observed the same thing in all attempts of this kind; and he therefore avoided them. He was convinced of a sort of rivalship between cohesive attraction and many chemical attractions, by which they counteracted and weakened each other. But this was only a way of conceiving, or rather of expressing, a plain fact; for when he considered them purely as attractions, there could be no opposition between them.

This is a very just account of the matter. Indeed, when we recollect that the united efforts of all the mathematicians of Europe have not yet given a demonstrable and accurate account of the motion of three particles attracting each other, we can expect little success when we want to determine the motion of millions attracting at once.

But, although we cannot, perhaps, explain phenomena by this way of considering them, we can discover, in some cases, the insufficiency of explanations which are in vogue. Nothing is more common than the notion that fluidity is produced by heat, merely by its destroying the attraction of cohesion. Observing that all the corpuscular actions are weakened by an increase of distance, and that heat expands bodies, we think it plain that the expansion may be continued till the particles get beyond the sphere of cohesion, and that fluidity is the necessary consequence. Dr. Black very properly observes, that the attraction of cohesion is not destroyed; because we see that a fluid gathers into drops. He grants that it is weakened, and seems to think that the attraction, weakened to a certain degree, is the constitution proper to fluidity. But this concession is not necessary, and this weakened attraction is not enough for fluidity.

It is by no means certain that the attraction of two particles of water is less than that of the same particles in the form of ice. The only effect of this attraction which can affect their motion, is their being pressed together by it, as a piece of iron is pressed to the magnet. Now, we know that water is equally fluid at the bottom of the ocean as at the surface. At the bottom, the particles are pressed with a very great force, probably equal, nay, certainly superior, to

the cohesion of ice. The easy separation of the particles of water is owing to this perfect mobility from one situation among the others to another situation. In shifting them from their places, we are scarcely opposed by any thing besides their inertia. We act but on one, or but on a very few at a time. It is quite different when we attempt to separate the parts of a solid. But to understand this difference, we must think a little more of the nature of fluidity and solidity. I do not mean to enter into the laws of attraction, &c. but merely to reflect on what circumstances are necessary for rendering an assemblage of matter a fluid, such as we observe water to be. A fluid is an assemblage of matter, the particles of which yield to the *smallest* impression, and, by thus yielding, are moved among themselves with the greatest facility. A body, having a base perfectly flat and smooth, and lying on a horizontal plane, having no friction, can be pushed to any side, in the same manner as if it hung in the air, whatever its weight and pressure may be, because it must be indifferent as to any situation on that plane. In like manner, in order that a particle of a fluid may be so easily moved from its place among the rest, all that is necessary is, that all new situations shall be perfectly indifferent. That this may be the case, the action of a particle must be equal in every direction at the same distance ; no matter how strong this may be, or whether it attract or repel. It is quite plain that if this action be thus equal in every direction, it can require no force to remove an adjoining particle from one situation to another. Nor will it require any force to keep the particle in this new situation, because this situation is indifferent to it, there being no inequality of action urging it to either side. The attraction and repulsion may vary according to any law, by a change of distance from the particle ; but it is equal in every direction at that distance. An assemblage of such particles must have all the well-known appearances of a fluid. It will gather into drops perfectly spherical, because any other form would cause an inequality of the compound action on any one particle, to which inequality of action the particle would yield, and it would shift its place. Such an

assemblage will, if heavy, acquire a horizontal surface, &c. &c. It may be demonstrated that such a particle must either be a single atom of matter, or must be a sphere containing numberless atoms *symmetrically* disposed. The spherical form is necessary for fluidity, not for the reason commonly assigned, that they may touch in few points, and easily slide on each other. These would not constitute a fluid ; for a parcel of spheres, perfectly smooth, could be piled up in a pyramid. It is necessary merely in order to produce this equality of action in every direction.

Solidity, on the other hand, requires a very different law of action among the particles. All situations must *not* be indifferent. A particle must affect a *particular situation* among the adjoining particles, must require some force to change its present situation, and when this is changed a little, it must have a tendency to regain it, and must actually regain it when the disturbing force is removed.

To effect this, it is only necessary that a particle attract or repel another more strongly in one direction than in another, at the same distance ; and if the action of a particle be thus unequal, an assemblage of such particles cannot be fluid. The particles will unite in one *attitude* rather than in another. Force is necessary for changing this arrangement ; and if this change be very small, it will be resumed when the force is removed.

All this will be illustrated by attending to the appearances exhibited by a parcel of small bits of iron wire, (suppose one-tenth of an inch long) scattered at random on the surface of quicksilver. They will lie in any position, and the touch of a single hair will change their position at pleasure. But now bring a great and powerful magnet near them, they will all instantly change their respective attitudes and positions. Each will turn its ends towards the ends of the adjoining particles,....they will run together into certain curious groupes. The mere touch of a hair will not change these new positions and attitudes ; or, if it should change them a little, they will resume them when the hair is withdrawn. The reason of all this is merely, that the neighbourhood of the magnet makes them all little magnets ;

and now each little bit attracts *more strongly* by its ends than by any other part, and in one direction much more than in another ; so that there is a particular position and attitude, which each will assume and maintain with a certain minute force.   Remove the magnet, and they all return to their former state of indifference and mobility.

The constitution of a solid body resembles this.   The particles have such an inequality of action, resembling the polarity of magnets.   It is by this, operating in the most favourable circumstances, that most solid bodies acquire a symmetrical arrangement of parts.   Thus do salts concrete from their solutions in a crystalline form.

Here then we have a *narration of facts* which cannot be disputed.   A fluid *must* consist of particles, which, while they attract each other, will take any situation indifferently.   A solid consists of particles which have a quality resembling the polarity of magnets ; and we understand in some small degree, on what dynamical principles these constitutions depend.

Now, let heat be applied to a solid body, and let it continue to expand it till its cohesive attraction is destroyed, what is the effect? It now is an assemblage of incoherent particles. But this is not a fluid ; it is a dust, an impalpable powder, which may be heaped up like so much flower ; or it is a parcel of slightly *adhering* particles, like wet sand, or half-boiled barley.   But it is not fluid.   Heat must do a great deal more to make it a fluid.   It must render each particle equally active in every direction, destroying its former polarity.

Thus, we see clearly that the particles do not first fall asunder, then take the arrangement of fluidity, with an enlarged capacity; and lastly, *admit* the heat into the enlarged pores.   The arrangement of fluidity requires the previous change of the law of action ; and this must be produced by the heat.   Therefore Dr. Black was fully warranted to say that fluidity was immediately effected by heat.

What has been said of fluidity is also true with respect to the vaporous form of aggregation, and need not be repeated.

We know very little of the corpuscular constitution of bodies ; yet we know something, and can make some useful in-

ferences. We know that all the corpuscular forces diminish fast by an increase of distance: and *we can demonstrate*, that the *inequality* of action on which solidity depends, decreases much faster. We see this remarkably in magnets. At a small distance the action of a magnet, in different directions, is very unequal; but as we recede, the inequality of action diminishes exceedingly. We can demonstrate strictly, that if a body, acting in this unequal manner, be surrounded by an assemblage of smaller particles, which act equally at the same distance at least, the inequalities in the action of the nucleus will be prodigiously diminished by the superadded equable action of this sort of atmosphere. Nay, we can shew that this atmosphere may be so disposed around the particle as to take away entirely the polarity or inequality of action. We see, therefore, that the accumulation of heat round a solid particle, must, independent of the expansion caused by it, diminish the cohesion of solidity, and cause the union to approach, at least, to the nature of the union of fluidity. The great quantity of heat that is accumulated round the particle in the liquefaction gives still more weight to this argument. We see, too, how the abstraction of heat, and the contraction of bulk, tend to bring the unequally acting parts nearer to each other, and to give occasion for the cohesion of solidity. When a mass of water has cooled below the freezing temperature, nothing induces the freezing with so much certainty as the touching of it with the point of one of the fine spiculæ which have been formed by freezing. It is evident that the polarity of this spicula, by its action on a particle surrounded by a small quantity of heat, must dispose the heat to emerge in fine streams from certain points of it, by destroying that equilibrium of heat around it, which fitted the particle for the fluid aggregation. I am far from offering this as a complete explanation; but I say that if heat consists of particles much finer than those of tangible matter, and if it be capable of being constipated round them, it cannot but produce effects of *this very kind*, and that it is possible for it so to accumulate round them, as completely to effect the changes that we observe.

The change from the liquid to the vaporus form is much more easily conceived, but does not afford such cogent proofs of the important part which is acted by heat in producing the change. But there is something curious in the law of expansion that seems to obtain in vapours. Equal augmentations of temperature (which seem to arise from equal additions of the matter of heat) produce nearly equal *multiplications* of bulk. This seems to indicate an equal density of heat in all temperatures. For, if the atmosphere of heat be like other elastic atmospheres, equal additions of the matter of heat should produce additions of bulk, which continually increase.

There is another circumstance in the vaporous combination of heat, which I think of immense importance to the philosophy of chemistry, namely, its being counteracted by mechanical force. Not only can the elasticity or mutual repellency be counteracted by pressure, but *the chemical union of heat and the matter of a body can be prevented by it*. The instant that the pressure on the surface of boiling water is removed, the union takes place; for steam is produced, and the temperature of the water is diminished. Hold the ball A of the pulse-glass represented by fig. 3. in the mouth. The liquor will boil in the ball B; and A will feel cold. Grasp B in the warm hand, and the ebullition is then stopped by the elasticity of the steam, which is allowed to retain this form by the warmth of the hand. A immediately grows warm also. Now remove the hand from B, and let it be wetted with some strong spirits of wine. This produces a cold which condenses the steam in B, removes its pressure, and the ebullition begins again. At the very same instant A feels vastly cold. Thus was the combination of water and heat in A effectually prevented, although the mutually acting substances are contiguous as before. Add to all this, that when the vapour is produced by the union of heat with tangible matter, pressure will put an end to it, the vapour becomes water, and the heat flies off. We cannot form a conception of any thing opposing a mechanical force, but another mechanical force. Hence we must infer that the corpuscular powers or qualities which ultimately produce the chemical phenomena, are not different from the forces which pro-

duce and modify local motion, and are really similar to those which produce the fall of heavy bodies, and the communication of motion by impulse. There were strong reasons for thinking so, independent of this fact. Capillary attraction, adhesion, cohesion, and some of the effects of electricity and of light, furnished surmises of this kind. But the prevention of vaporization by external pressure is the most direct proof. The conjecture of Sir Isaac Newton, expressed in one of his reflections at the end of his treatise on Optics, seems fully verified ; and it becomes highly probable that all bodies consist of atoms endowed with one and the same power of attraction and repulsion, varying according to a determined law of the distances. The ingenious essay on this subject by the Abbe Boscovich merits therefore the careful perusal of every philosophical chemist.

[*Note* 6. *p.* 196.]

There is something very curious and unaccountable in the manner in which these vapours are precipitated. It is not in round drops, as one should naturally expect. Were this the case, the precipitation would be almost instantaneous. A drop, whose diameter is the thousandth part of an inch, would acquire the velocity of nine or ten feet per second ; whereas we see clouds hover at a very small elevation, for many hours, and they can thus be transported from the sea, lake, river, or marsh, from which they are raised, far into the country, where such supply cannot be had any other way. Thus is water carried to the tops of mountains, where it is condensed, supporting their vegetation, and giving the surplus to the ground, there to percolate, and supply us with springs and streams of fresh water. A cloud formed of solid drops would always exhibit a rainbow when opposite to the sun ; and when between us and the sun, would exhibit coloured rings altogether unknown in the ordinary course of things.

The precipitated or falling mist is all in the form of thin transparent bladders or vesicles. This was affirmed by Der-

ham and others, but without any authority, or indeed any assertion that they had observed them. M. Saussure, in a journey of observation among the Alps, found himself in a thick mist, which was almost stagnant. He was astonished at the size of the drops, as he thought them, and at seeing them swimming slowly by him without falling to the ground. Some of them were larger than the largest pease. Catching them in his hand, he found them to be nothing but bladders inconceivably thin. He now found them all to be of this structure. He is the first person, so far as I know, who has described them. I have many years ago observed that the particles of mist visible in the receiver of an air-pump, are bladders. I inferred this from their size, and from their very slow descent through air so much rarefied. I was much surprised at them, but imagined that this was peculiar to the precipitate formed by sudden rarefaction. But I now see that M. Saussure's description is general. The mist of boiling water, when between the eye and a candle, and between us and the sun, exhibits the halos which are producible by vesicles only. This is easily demonstrated by the laws of optics

This constitution of a mist or fog, so necessary for accomplishing the great purposes of nature, is to me most unaccountable. I cannot conceive how the formation of such a vesicle commences, or what is the aërial substance which distends it. It is an air in a state of slight compression; for the film of water round it is endeavouring to contract. I see such bubbles sometimes arise from lintseed oil, when boiling to free it from its watery part. This I understand; but its origin has no resemblance to the production of a mist. The vesicles may be very distinctly seen in the evaporation from a dish of very warm coffee without milk. Set it in the sun, and, in viewing it, turn the back to the sun. When thus viewed through a magnifying glass, the vesicles are seen rising in great abundance; some of them so large and heavy, that they fall down immediately, and roll about like so much dry dust on the surface of the coffee, appearing like brilliant spots

on the dark ground.   *(See M. Saussure's Essai sur l'Hydrom-etrie, page* 285. *)*

[*Note* 7. *p.* 202.]

If heat be the sole agent in producing the vapour, I cannot see how the air above the evaporating body can be uniformly transparent.   Although the vapour may have the same elasticity with the air, it is very improbable that it has either the same weight, or the same refracting power, and it must always surround the evaporating body with an unequable atmosphere, like a mixture of oil and water shaken together. The necessity of renewal of air, the stop to evaporation by stagnation and even the effect of extended surface, are all inexplicable, if heat alone be the agent.   These facts are strong arguments for believing that the air *also* acts by its dissolving power. The prevalence of bad smells immediately before rain, is a fact very similar to the precipitation of one substance by means of another.   Dr. Hutton's observation, that two masses of very damp air, of very different temperatures, precipitate water on mixture, is a fact similar to a like precipitation of nitre from two saturated solutions of different temperatures, but is inexplicable by the other theory.   Dr. Black thought very highly of M. Saussure's *essais sur l'hydrometrie*, and of his general method of philosophising, which is perspicuous, logical, and modest.

[*Note* 8. *p.* 207.]

I do not think that I can conclude this article as I ought, without taking notice of some facts and circumstances, which affect Dr. Black's claim to be considered as the discoverer of the peculiar combination of heat, or the cause of heat, with different substances, by which they are made to exist in the form of a liquid or of an expansive vapour.   His title to this discovery, and to the theoretical doctrine founded on it, has been called in question.   I am not certain that it is generally acknowledged on the continent, or that it is even generally

known that he has occupied himself with this research. I see it treated in the compilations of many of the foreign chemists, with more or less ability, as the discovery of Mr. Wilcke of Stockholm, and by others as coming from Mr. De Luc, and by many as a part of the great discoveries of Mr. Lavoisier. I find this last opinion very general, and I see clearly whence this arises. Mr. Lavoisier and Mr. De la Place published the first of their valuable dissertations on the relations and measures of heat in 1782. Of the doctrines contained in these dissertations, this doctrine of *combined* (or, as Dr. Black calls it, *latent*) heat, is the basis, and it is involved in all the chief propositions established by these philosophers. Yet the name of Dr. Black is not mentioned. These dissertations seem to be received as the original source of the whole doctrine, and to be considered as the result of *their* labours alone.

Although Dr. Black was duly conscious of his own merit as a chemist and a philosopher, there never was a man less assuming, or less eager to obtrude himself on the public attention. Even his discovery of fixed air, and his ingenious theory of quick-lime, would not perhaps have been committed to the press, had they not formed the subject of his inaugural dissertation, when he received his medical degree. His doctrine of latent heat, and the theory of its combination with all kinds of matter, in the forms of liquid and of vaporous aggregation, although incomparably more ingenious, and of more extensive and important application to the explanation of natural phenomena, were known to the world only through the lectures delivered to his own numerous students. Scarcely did a year pass over, after his settlement in Edinburgh, in which some corner of this doctrine was not nibbled off, and appropriated by others ; generally by his former pupils. He was often urged in the most pressing manner by his friends to publish, and was told that the whole of it would, in this way, be soon honestly portioned out among different persons, as the fair result of their own enquiries; and that if he should at last publish, he would only be considered as a copyist. Yet nothing could prevail on him (so much did he dislike authorship) to vindi-

cate his claims, till he learned that his friend Mr. Watt was
likely to suffer by his silence, because others were handing
about as their own, some very peculiar views which he had
taken of the subject, dependant on Dr. Black's doctrine. The
Doctor was so far roused by this, that he promised to Mr.
Watt that he would print an account of these speculations with-
out loss of time. But this went no further than writing two
octavo pages, and noting with his pencil some particulars for
continuing this account.

In the mean time, others were appropriating to themselves
various parts of the doctrine of latent heat, or, without per-
ceiving the connection of the whole, were acquiring reputation
by ingenious explanations of particular phenomena, which de-
pended on this doctrine. Thus, in or about the year 1772,
Professor Wilcke of Stockholm read in the Royal Society in
that city, a dissertation, in which he shewed that ice absorbed,
while melting, 72 degrees (by the Swedish scale) of heat.
Magellan published a work, in which he asserts, in a very au-
thoritative manner, that Wilcke is the author of the discovery.
Others represent Dr. Cullen as the author, because his sur-
prising experiments with æther *in vacuo*, exhibit the most re-
markable example of the absorpsion of heat. But Dr. Cullen
never laid the smallest claim to the doctrine. Many affirm
that Dr. Irvine and Dr. Crawford share at least the honours
of the discovery with Dr. Black, and that they had more just
conceptions of the phenomenon than their preceptor.

The claims of these gentlemen are however, refused by that
eminent naturalist Mr. De Luc, in his *Meteorologie*, publish-
ed in 1786. But this refusal is not in order to secure the dis-
covery to its worthy and unambitious author. Mr. De Luc
condescends, indeed, to say that *Dr. Black was the first who*
ATTEMPTED *to measure the quantity of heat that is absorbed du-
ring the melting of ice and the boiling of water.* But he consi-
ders himself as the first author of the doctrine that *heat com-
bines with the particles of a solid body, and that the result of
this combination is the liquefaction of the solid, and its exist-
ence in a fluid form; heat or fire being one of its constituent
parts; and that, in like manner, another combination of fire*

with the particles of bodies converts them into elastic vapour, of which fire is a constituent part. He refers the reader for proofs to his publication in 1772, under the title of *Recherches sur les modifications de l'atmosphere*, of which this theory, modified in a very particular manner, makes a principal part. He professes to have formed this theory in the years 1754, 1755, and 1756, and quotes some observations or experiments, of that date, on which he affirms it to be founded. He considers Dr. Black's doctrine of latent heat as making but a part of his own theory, which extends to every case of evaporation, while Dr. Black's relates only to the heat absorbed in ebullition, and combined in elastic steam.

Mr. Watt, warmly attached to his highly respected friend Dr. Black, was surprised at this faint and inadequate concession of Mr. De Luc, and was grieved that the Doctor's inactivity had given room for it. Knowing that every publication of so eminent a philosopher as Mr. De Luc would strongly impress the public mind, and would leave it uninformed of what Dr. Black had done, he wrote to Mr. De Luc a letter, in which he asserts that Dr. Black had done much more than *merely* TRY *to measure the quantity of heat absorbed, &c. ; that he had first discovered and demonstrated that heat is absorbed, and is combined with bodies when they are rendered fluid or vaporous, &c.;* and he insisted that this letter should be inserted, at the end of the next volume of the *Meteorologie.* Accordingly it is there inserted, in page 503 of the appendix. Mr. De Luc expresses his regret that his words had been taken in a sense different from that in which he had employed them, and professes the highest regard for Dr. Black. But he still retains his claim to discovery and to priority.

There is nothing of which I am more fully convinced than that Dr. Black is the *sole author* of the doctrine of latent heat ; and, when the following particulars, leading to that conviction, which my duty to my much respected teacher and friend impels me to lay before the reader, are duly considered, it will remain with him to say whether he coincides with me in the belief, that whatever portions of this doctrine may have been seen, or guessed at, on the continent, or in these kingdoms.

they have emanated originally from Dr. Black, probably through the medium of students attending his lectures, as they did from every part of Europe.

Dr. Black was so little solicitous to secure his title to discovery, that I got very little information to this purpose among the notes from which he lectured. But, searching among his old memorandums and discarded papers, I found several small note books of an uncommon form, such as I remembered to be much used by the students in the University of Glasgow, and were sold in the shop of the University printer. These little note books were filled with memorandums of things which had engaged the attention of the young philosopher ; and these were mixed with notes of occurrences, and names of persons, which were all familiarly known to me, I being a native of the place. By these circumstances, the dates of many memorandums may be ascertained within a month ; and they shew the whole to have been written in 1754-5-6-7, while the Doctor was a student at Glasgow and Edinburgh, and during the first year of his Professorship in Glasgow. I shall insert a few of such notes as regard the present subject.

No. II. Page 1. " Does not the heat produced in slacking " lime, and in the solution of lime and caustic alkali in acids, " arise from the sudden fixing of the fluid part in a firm state ? " taking them from a state of fluidity.

Page 2. " Intense cold produced by ice dissolved in an " acid explained, and the principle applied to crystallization. " Is not ice crystallized water ? Is it not similar to a solution " of a saline crystal ? A greater alteration of heat should be " produced by mixing a neutral salt with ice or snow, than by " dissolving it in water.

No. III. Page 1. " One way of estimating the quantity of " heat which is transferred and disappears in the production " of vapour, or which is expended, and contributes to the for " mation of vapour, but cannot be discovered in the vapour " by the thermometer.

" Place a phial with water, close corked, in a stove or oven, " where it will acquire a degree of heat above the boiling " point : Then, by some contrivance, open it suddenly, and

" observe how much has been converted into vapour, and what
" heat it retains."

N. B. In the net page Dr. Black describes the method of ex-
amining the thermometric scale, so as to decide whether the
scale of equal expansions corresponds to equal augmentations
of heat. Mr. De Luc seems to consider himself as the first
who examined this question also.

No. IV. Page 6. " Nitrous æther boils *in vacuo*, and
" grows vastly cold. Is not the cold produced merely by
" the evaporation of the elastic parts ; or does it emit air,
" which requires heat for its elastic appearance ?

Page 20. " In the effervescence of mercury in nitrous acid,
" whence comes the great heat which must certainly be re-
" quired ; for so much. elastic matter must rob it, as æther
" boiling *in vacuo* does ?

" All solutions must be considered as liquefactions in con-
" sequence of heat, for there is no fluidity without heat. Salt
" and water, when mixed, melt with much less heat than what
" will melt ice, and vastly less than what will melt salt.

Page 30. " Does it not appear, on a review of cooling
" mixtures, that a refrigeration will always happen, when salt,
" in a crystalline form, (that is, combined with water) or water
" in a crystalline form, (that is, frozen) is made fluid by salts,
" or in spirit of wine. According to this supposition, salt
" mixed with ice should produce much more cold than salt
" mixed with water that is as cold as the ice.

" Will not the progress of cooling be retarded while a solu-
" tion of salt crystallizes? Try if this happens while fats be-
" come solid.

" Do not these salts produce the greatest cold which are
" dissolved by the smallest quantity of water ?

Page 32. " Will a boiling fluid raise the thermometer in
" proportion to the pressure on its surface ? Will this be of
" any use in discovering the absolute proportions of heat ?

No. V. " Much greater heat when a caustic ley is mixed
" with an acid, than when a mild one is ; none is taken off by
" the elastic effervescence.

Page 15. " Will distillation *in vacuo* save fuel ?

Page 16. " Is not heat produced in solutions, when a fluid
" matter is converted into a solid ? Calcined Glauber salt and
" water ; but not when solid matter is rendered fluid.

Page 24. " Is not the cold produced by mixing vitriolic
" acid with sal ammoniac owing to the production of elastic
" matter ?

No. VI. " Is the heat which is disengaged from water in
" becoming solid in a crystal ; equal to what must be employ-
" ed to disjoin it from the water ? How may this be tried ?
" In a salt it is not all disengaged, for the brine is still salt ;
" and when it effloresces, more must be employed to produce
" vapour."

By these memorandums, I think that it must be evident to
any reader, that the doctrine of latent heat, as a necessary
ingredient in all fluids and elastic vapours, has not been an
opinion suddenly formed by Dr. Black, or deduced from a
single phenomenon.   Nor has it arisen from a general, but
vague view of all the phenomena of liquefaction and vapori-
sation. It has long occupied the mind of the ingenious author,
and his surmises have been noted down, that they might not
be forgotten, while present studies, or the circumstances of
his situation, hindered him from putting his conjectures to
the test of experiment. He might truly say, therefore, as in
page 156, that he scarcely remembers the time when some
suspicion of this combination did not possess his mind.   The
theory has been the *matured* result of many observations, and
frequent meditation.   The reader will not wonder that Dr.
Black's mind was not sooner made up upon this subject,
when he recollects that he was elected Professor of Medicine,
and that chemistry was only a lectureship.   His medical stu-
dies must have occupied most of his time ; and the attention
which he gave to his patients was so anxious and unremitting,
that one should have thought that he had not an hour left for
any other duty.   Mr. Watt thinks that he had completed his
proofs of the absorption and emersion of heat in liquefaction
and congelation in 1758.   But, in an old and rejected sheet
of lectures, which some other circumstances oblige me to re-
fer to this year, he treats the subject still as a general view,

and makes no mention of the *quantity* of heat which enters into the formation of water. I think that this would not have been omitted, had the experiments been already made. I am persuaded, however, that the extension of the doctrine to the ebullition of fluids followed very soon after its establishment in the cases of liquefaction and congelation. The fundamental phenomena, and the experiments for determining this part of the theory, appear to me to be much more obvious and impressive than in the other case. I am so much of this opinion *new*, that I wonder, as often as I think of it, that the heat latent in steam was not detected even ages ago. At all events, it is fact, that the doctrine of latent heat, precisely as it is contained in these lectures, was explained and demonstrated by Dr. Black in 1761, and in every subsequent course.

I must now take the liberty of saying that I was particularly hurt, when I was informed of the cold, and, as it were, reluctant concession of Mr. De Luc, "*that Dr. Black was* " *the first who had* ATTEMPTED *to measure the quantity of heat* " *which ice absorbs during its liquefaction*," &c. because I had long been made to expect from Mr. De Luc a very different account indeed of Dr. Black's labours. At this time (1786), I had been for several years the colleague of my former preceptor, and I took the liberty to join in the importunities of his friends, who urged him to print an account of his doctrines. His health was very delicate about this time , and we despaired of his being able to undertake the task. We were consoled, in some degree, by hearing that Mr. De Luc had *repeatedly* offered to take this trouble on himself. We were given to understand that, three or four years before this, the Doctor had been prevailed on to communicate the necessary materials to that gentleman for this purpose, and that the account would speedily appear, in a great work which he was about to publish ; and it was said that Mr. De Luc was particularly desirous to have this opportunity of maintaining Dr. Black's right, in opposition to the pretensions of Dr. Crawford and others, who had endeavoured to rob him of his deserved fame.

The account which did at last appear was indeed a matter of surprise and disappointment: For it was understood that Mr. Watt had been allowed to communicate to Mr. De Luc the leading points of his theory, and even to repeat with him the experiments necessary for its demonstration. Yet not a word is said by Mr. De Luc of the attempts of Wilcke, Dr. Crawford, and Dr. Irvine, to explain the phenomena in another way, except in order to refute them by his *own* NEW *theory of watery vapours.* Nay, the explanation which Mr. De Luc vouchsafes to give, after inserting Mr. Watt's letter, and acknowledging that his words were susceptible of the interpretation given of them, is merely condescending to say that *it was a great satisfaction to him to learn, by the letter of Dr. Black's friend, Mr. Watt, that his own system of the combinations of fire, as a constituent part of certain substances, had had Dr. Black for a defender at that time, as it has since had in Mr. Lavoisier ; and that he will have a greater confidence now in his opinion, since it is common to him with naturalists of their rank.*

This is really too much. Had Mr. De Luc forgotten his repeated offers and requests to become Dr. Black's editor, and his importunity to be instructed in the particulars? Had he forgotten his being a witness, in 1782, of some of the principal experiments on the latent heat of fluids, when boiling under the pressure of the atmosphere, and also *in vaeuo*, and the important deduction made from them by Mr. Watt?

Let it not be thought that I make these assertions respecting Mr. De Luc's acquaintance with Dr. Black's doctrine, on the sole authority of what I heard in conversation with Dr. Black's friends. I have now lying before me the unfinished scroll of a long letter of Dr. Black's handwriting to Mr. Watt, in which every particular that I have mentioned is alluded to. Mr. De Luc's repeated solicitation to be his editor,....his intention of doing him justice, by supporting his claims against the encroachments made on it,....assurances from Mr. Watt that Mr. De Luc will be a zealous friend,.... Dr. Black's expressions of confidence in Mr. De Luc's genius and honour,....specification of the things to be communica-

ted, and the experiment to be shewn him. The letter is da-
ted January 30, 1783, but is not finished.

Let us now consider a little the authority on which Mr. De
Luc founds his claim to the discovery of the absorption and
combination of heat or fire with the particles of other matter,
so as to become a constituent part of every fluid and of every
vapour.

For this the reader is referred to the *Recherches sur les Mo-
difications de l'Atmosphere*, published in 1772. The authority
referred to in that work for the latent heat of melted ice, is
an observation said to have been made in 1754, *that there
was no rise of temperature in a vessel containing ice melting,
till the whole was become fluid.* I believe that this has been no-
ticed by every naturalist, since the time that Dr. Hooke re-
commended melting snow for settling a point in the scale of
temperatures; for the propriety of this method of ascertaining
a determinate temperature rests entirely on the truth of this
observation. Both Fahrenheit and Mairan, after remarking
that the temperature rises to the melting degree, affirm that
it remains fixed there till all be melted. But neither of these
philosophers infer from this fact that the ice is continually *ab-
sorbing heat*, and that the heat so absorbed may be measured
by the time of the liquefaction. Neither does Mr. De Luc
make any such inference: *Nor does he afterwards employ this
inference as a principle in explaining any phenomenon.* Nor
does he shew how this absorbed heat may be made to emerge
again, and warm other bodies.

This inference, this conclusion drawn from the constancy
of the temperature, was reserved for Dr. Black; and he
proved that it was a just inference, by shewing that the same
quantity of heat is afterwards obtainable from the water, when
it freezes. Mr. De Luc noticed the fact indeed, that, during
the liquefaction, the temperature was constant: But it took
no hold of his mind. It seems to have been forgotten till
1772. I do not see therefore that he can found his opinion
or his theory on this observation. It does not receive or ad-
mit any explanation from his singular theory of watery va-
pours.

How different is all this from the procedure of Dr. Black? The notes which I have inserted shew that a certain opinion had long lurked in his mind concerning the constitution of fluids ; and that from time to time it had gathered strength, in consequence of his observing classes of phenomena, which would be just what we see them, on the supposition that heat is a constituent part of all fluids, and that fluidity is the effect, and the only sign of this particular combination of heat with other matter. The proposed experiment with the corked phial is the one which was made long after by Mr. Watt, with Papin's digester. The conjecture about the intense cold in freezing mixtures is precisely what was afterwards found to be just. In short, Mr. De Luc's observation was just one of those remarks that are frequently made by an ingenious man habituated to speculation: but it lay for years unimportant and neglected, among other memorandums ; whereas there was a prepossession, or an impression on the mind of Dr. Black, which directed his attention to certain classes of facts, and was nourished and strengthened by subsequent remarks and reflections, till a great body of evidence at last incited him to a serious discussion, which terminated in conviction.

I have already said that the arguments for the vaporous combination of heat are much more obvious and impressive. I am not surprised, therefore, that so ingenious and inventive a mind as that of Mr. De Luc very early conceived this with some degree of confidence and precision; and yet, even on this head, Mr. De Luc is satisfied with *general observations*. In § 676, he says distinctly enough, that " *Fire quits the fuel* " *to unite with the water, converts it into a vapour, and escapes* " *with it.*" Farther, in § 684, he generalizes the fact, by *ascribing all evaporation to the union of fire with water ;* and in § 693, he explains on this principle the cold produced by evaporation. But, as Mr. Watt very properly observes in his letter, Mr. De Luc has made no experiment to demonstrate any part of this theory, or to determine how much heat is absorbed or emitted. Surely, till something like this be done, the whole theory is nothing more than an ingenious but vague

conjecture, resting entirely on general observations, and des-
situte of that precision which is required for the foundation of
a theory.   Muschenbroeck had formed the same conjecture
long before (See his *Natural Philosophy*, translated by Sigaud
de la Fond § 1455, &c.)   He says, that *the fire enters the*
water, throws it into ebullition, and that the greatest part of the
igneous matter escapes ; but a portion remains combined with
the water, and gives the vapour its expansive form and force.
As for the accessary circumstances of Mr. De Luc's theory,
and his manner of conceiving fire as an *expansive fluid*, acting
the part of a carrier to the water, and the mode of its efficiency
in taking up or depositing the water with which it is com-
bined, I admit that *this is the entire property of Mr. De Luc.*
I content myself at present with agreeing with Mr. Watt, that
no experiment is adduced by the author, by which any part of
the theory can be said to be demonstrated.   I have not the
least notion how this theory of vapour is to be connected with
that of liquefaction.

In the mean time, I request the reader to recollect that the
Recherches, &c. were not published till 1772.   The main
question is, whether Mr. De Luc held this doctrine in 1754
or 1756?   I rather think that he did not.   He only says
(Meteorologie, app. p. 512.) that in his *Recherches* he made
use of observations which he had made in 1754 and 1755, on
the phenomena exhibited by freezing water and melting ice,
and of those made in 1756 on vapours : and he acknowledges
that, with respect to the first, he went no farther at that time.
Now, the proofs of absorption, combination, and emission,
and the mode of reasoning from those proofs, are so similar
in the two cases, that I cannot conceive how a naturalist who
draws conclusions from the one does not find himself incited,
nay obliged, to make the same inferences from the other.   I
presume that Mr. De Luc, incessantly occupied with meteoro-
logical studies, has been considerably later in forming the
theory, explained *in some degree* in the researches, and has
found that these old observations or remarks also were ex-
plained by it, and might therefore be adduced as proofs.   But,
can a person be thought to have any clear notions of the sub-

ject, who says in § 676, that " water extinguishes fire, because
" it has an affinity with it; and that air on the contrary ani-
" mates fire, because it has no affinity with it?"....who says, that
" by the pressure of the air, the fire is confined in the com-
" bustible or the hot body, and kept in prison, but, when this
" compression is removed, the fire immediately darts out,
" and is dissipated?"....who says, § 684, " that the particles
" of fire, being in continual agitation, knock on the particles
" of bodies, separate them, and in their escape from the body,
" carry off some watery particles with them, and thus become
" the vehicle of the water, and constitute what we call watery
" vapour?"   This is in no respect different from the vague
writing of Muschenbroeck, who is always groping about, and
catching at any thing that will bear an application to his sub-
ject; and then gives us a strange mixture of mechanism and
chemical affinity, which both mechanicians and chemists will
only smile at.   By collecting such vague and occasional ob-
servations of the ancients, Mr. Dutens found in their writings
all the discoveries of the two last centuries: and Mr. De Luc
may find in his *Recherches sur les Modifications de l'Atmosphere*,
every discovery made since 1772.   Such writing, by its want
of precision, becomes a net which will catch both fishes and
fry.   Nothing can escape it.   And shall this be compared
with the clear and precise propositions of Dr. Black?....These
may be false; but we know exactly what they are, and can
form the most distinct notions of them, so as to subject them
to the test of experiment, which will at once establish or refute
them.

I have said that the *Recherches* did not appear till 1772.
Dr. Black ere this had given at least ten or twelve annual
courses, in which his doctrine of latent heat was demonstrated
and explained.   He had many pupils from Geneva, some of
them very well informed young gentlemen, and very able to
understand this doctrine.   I remember a Mr. Chaillet at
Glasgow in 1763, and a Dr. Odier in Edinburgh, who (at
least the latter) corresponded with Mr. De Luc, and commu-
nicated several of his valuable observations to Dr. Black.
But we never heard of such opinions being held by any other

person besides our own professor. I should think it not un-
likely, that a philosopher of Mr. De Luc's ardour and assi-
duity, who held a correspondence with the learned in all parts
of Europe, would be pretty soon informed of Dr. Black's
speculations. Indeed there was a surreptitious and imperfect
publication made of them at London by *Nourse* in 1770, en-
titled *Enquiry into the General Effects of Heat, with Observa-
tions on the Theories of Mixture*, in which all the general or
leading points of this doctrine were expressed with abundant
distinctness and precision ; and the honour of the invention is
given in very express terms, to Dr. Black, professor of chem-
istry at Edinburgh ;...." a man," says the compiler, " whose
" modesty is even superior to his merit." These speculations
might find their way to Stockholm in the same manner. I re-
member a very accomplished young gentleman from thence
in 1768 or 1769, (I think his name was Willems or Willem-
son) who was much in the company of Dr. Black and his
friends. He was occupied solely with chemistry, and parti-
cularly with metallurgy. Besides, manuscript copies of Dr.
Black's lectures were to be had for a moderate price, and were
purchased by many students.

Should I be thought by some readers to have trespassed
unreasonably on their time and patience, I sincerely ask their
forgiveness. But I trust that I shall have the thanks of all
those who have had the happiness of attending the lectures of
our amiable and excellent Professor. I hope also that others
will grant that I have only endeavoured to discharge my duty
to his honoured memory. A more proper occasion will pre-
sent itself, in the consideration of another article, for taking
some notice of the silence of Mr. Lavoisier, and Mr. de la
Place on this head ; when I trust it will appear in what high
estimation Dr. Black's speculations on this subject were held
by those gentlemen, and how anxious they were to keep them
from the public eye.

[*Note* 9. *p.* 208.]

This, however, is a very gratuitous opinion, and has always been admitted with great hesitation. Dr. Hooke exhibited at a meeting of the Royal Society in 1682, an experiment, in which it appeared that a mirror of foiled glass reflected the heat of a fire very feebly, although it reflected the light with great brilliancy. Mr. Scheele shewed that a plate of clear glass, held between the face and a glowing fire, interrupted for a while the whole heat, but transmitted the light without sensible diminution: And Dr. Herschell has shewn, in the most convincing manner, the complete separability of light and a determinate portion of the heat, by means of a difference in their refrangibility.

[*Note* 10. *p.* 210.]

Dr. Darwin directed a stream of air through a small aperture of a tube intensely heated, into a box, which had an optical apparatus in the opposite side, by which he could see any light at the point of the pipe with great advantage. Having staid some time in the dark, to render his eye very sensible to light, he looked in, and caused the injection of heated air to be made; but he could not perceive the smallest light. He then placed a slender bit of gold at the distance of half an inch from the hole. When the heated air was again injected, the gold acquired a very bright glow in a few seconds, although the heating air was altogether invisible. Dr. Herschell's curious observations shew us that, by a proper optical apparatus, a body may be intensely heated by the rays of the sun, without being at all illuminated; and it seems to require an experiment like Dr. Darwin's to decide whether *this* heat would cause it to emit light. Also, seeing that it is affirmed by the French chemists that oxygenous gas is the sole source of the heat and light which appear in combustion, it would seem to be doubtful whether Dr. Darwin would have seen the gold ignited, had it been heated by a stream of azotic gas instead of common air.

[*Note* 11. *p.* 214.]

This porosity of clay is susceptible of considerable variety,
by different modes of preparation. It is prepared for the
better kinds of pottery, by beating it into a uniform, and al-
most fluid mud with water, and allowing this to settle at lei-
sure in a pot or cistern. By this treatment, all small stones,
and even coarse sand, fall to the bottom. When so much of
the water has evaporated as to leave the clay fit for being form-
ed in the mould, or on the wheel, and able to support itself
in shape, it may be dried in the air and sun, without shrinking
remarkably. When a vessel of this consistency is baked with
a moderate red heat, such as is given to bricks for inside work
it is so porous that the water will run through it in a few mi-
nutes, but without taking away the smallest portion of it. This
is very easily understood. It is owing to the great space which
had been occupied by the water. The subsequent contrac-
tions of such baked clay by stronger heats are much greater
than those of clay which has been compressed by beating or
kneading, after a greater portion of the water has evaporated
from the prepared mud. I find, by some careful experiments
with the finest clays, that a difference in this respect induces
a very sensible difference in the total contraction of baked clay,
even when very hard. I mention this circumstance, because,
though sufficiently obvious and intelligible, it does not seem
to have been duly attended to by Mr. Wedgewood and others
who employed this kind of thermometer ; and it introduces
considerable uncertainty in the comparison of experiments
made by different persons. The different contractibility has
a sensible relation to the specific gravity of the baked clay,
(varnished, to prevent imbibition in the trial), but I have not
been able to ascertain its degrees with any tolerable precision.
I found very unexpected anomalies.

[*Note* 12. *p.* 227.]

Having occasion, in autumn 1774, to go down and inspect
a drain in a coal-work, where an embankment had been made

to keep off a lateral run of water, and, crawling along, I laid my hand on a very luxuriant plant, having a copious, deep-indented, white foliage quite unknown to me. I inquired of the colliers what it was? None of them could tell me. It being curious, I had a sod carried up to the day-light, to learn from the workmen what sort of a plant it was. But nobody had ever seen any like it. A few days after, looking at the sod, as it lay at the mouth of the pit, I observed that the plant had languished and died, for want of water, as I imagined. But looking at it more attentively, I observed that a new vegetation was beginning, with little sproutings from the same stem, and that this new growth was of a green colour. This instantly brought to my recollection the curious observations of Mr. Dufay ; and I caused the sod to be set in the ground and carefully watered. I was the more incited to this, because I thought that my fingers had contracted a sensible aromatic smell by handling the plant at this time. After about a week, this root set out several stems and leaves of common tansy. The workmen now recollected that the sods had been taken from an old cottage garden hard by, where a great deal of tansy was still growing among the grass. I now sent down for more of the same stuff : and several sods were brought up, having the same luxuriant white foliage. This, when bruised between the fingers, gave no aromatic smell whatever. All these plants withered and died down, though carefully watered, and, in each, there sprouted from the same stocks fresh stems, and a copious foliage, and produced, among others, common tansy, fully impregnated with the ordinary juices of that plant, and of a full green colour. I have repeated the same experiment with great care on lovage, (*levisticum vulgare*) mint, and caraways. As these plants throve very well below, in the dark, but with a blanched foliage, which did not spread upwards, but lay flat on the ground : in all of them there was no resemblance of shape to the ordinary foliage of the plant. All of them died down when brought into day-light ; and the stocks then produced the proper plants in their usual dress, and having all their distinguishing smells.

From such experiments, I thought myself entitled to say
that the sun's rays not only produced the green fæcula of
plants, but also the distinguishing juices, and particularly the
essential oils.   The improvements which have been made in
chemical science since that time, have, I think, fully con-
firmed my conjecture.   The separability of light and heat,
first noticed by Dr. Hooke, and exhibited by him to the
Royal Society, at the meeting of March 16, 1682; (See
Birche's Hist. of the Royal Society, vol. IV. p. 137.) and, af-
ter being forgotten, again brought forward by Dr. Scheele,
has been lately confirmed in the most complete manner by Dr.
Herschell's valuable experiments, in which he found the calo-
rific rays less refrangible than the lucific.   It was no unreason-
able supposition, therefore, that the sun's rays contained
phlogiston, since we see that they contained more than light.
Nor was my conjecture without support from what was then
known.   The sun's rays blacken the vitriolic acid ; and from
vitriolic acid much blackened we can obtain sulphur. His rays
render colourless nitrous acid ruddy and fuming, in the same
manner as a drop of spirit of wine would have done.   They
destroy vegetable colours.   They blacken luna cornea, and
other metalline salts or oxyds, in the same manner nearly as
fatty steams and vapours do.   These are strong indications
of some material emanations from the sun, and are in no man-
ner explicable by any mechanism of elastic undulations. These
phenomena cannot be conceived by the mind as the effects of
an undulation.   They resemble communications of matter.
But, indeed, the proportionality of the sines of incidence and
refraction has always appeared to me to give the strongest
proof, that the sun's rays are emissions of moveable, inert
matter, affected, as all other matter is affected, by what we
call moving forces.

The new doctrines in chemistry make no change in the in-
ferences from these phenomena. It is not admitted that phlo-
giston is communicated to nitric acid by the sun's rays when
they render it fuming ; but they are said to detach oxygen.
The whole train of chemical phenomena induce us to conclude
that oxygen is disengaged, either by being detached by some-

thing now combined with the remainder, or that the thing furnished by the rays makes part of the gas now formed.

When I first heard of Scheele's curious experiment of throwing the prismatic spectrum on the nitrate or muriate of silver, in order to compare the blackening powers of the differently colouring rays, it revived my chemical speculations, from which I had long desisted; and, in beginning to repeat his experiment, an accidental circumstance suggested to my mind an experiment which I thought decisive of the question, "Is light an emanation of matter from the sun, or is it the undulation of an elastic medium?" I proposed to form an image of the sun on the nitrate of silver, by means of rays collected by a metalline speculum, and made to pass through a glass vessel filled with clear and colourless nitrous acid. On the supposition that the preparation of silver is blackened, and nitrous acid rendered fuming, by the same principle in the sun's rays, I concluded, that when they had performed one of those tasks, they could not be in a condition to perform the other, at least in the same degree. I accordingly began the experiment, and found a remarkable diminution of the effect by the interposition of the acid. But I could not make any very confident inference from this, having long before that time found that the separation of heat and light by a transparent body, such as Scheele's pane of glass, was chiefly owing to absorption of the heat by the glass, which, after it was saturated, transmits the heat copiously, along with the light. It therefore required a train of experiments to determine how much was owing to this cause. Before I could accomplish this object, the failure of my health, in the spring of 1787, put a full stop to all active investigations and pursuits.

My thoughts on this subject have again been roused by the observations of Messrs. Bockmann and Ritter, in Germany, and Mr. Wollaston in England. They have observed that the muriat of silver was strongly darkened by rays of the sun which are more refracted than even the violet rays. The muriat was affected in a space lying beyond the violet light, in the same manner as Dr. Herschell had observed the thermo-

meter affected in a spase equally in the dark, beyond the red
rays of the spectrum.

Thus it appears that there are rays which illuminate, rays
which warm without illuminating, and rays which affect bo-
dies chemically without producing either light or heat. This
observation bids fair for giving us more knowledge of the na-
ture of combustion.   And, if this newly discovered principle
shall be found necessary for this most remarkable phenomenon
of nature, it may very well be called *the phlogiston*, in what-
ever way it contributes to the effect. And these facts seem to
recommend the prosecution of my experiment.   It would be
proper to compare the darkening power of the rays coming
through nitric acid with that of rays coming through as much
water.   The rays produce a remarkable effect on the first,
and none on the last.

But, whatever may be the result of such experiments, it
remains certain, that the rays of the sun, and the light of day,
affect plants in the manner described above ; and in some way
or other are necessary to the production of their most com-
bustible ingredients.   The plants may be said to feed on the
light by their green leaves, as much as they do on the juices
of the soil by their roots.   As the roots are protruded all
around through the yielding soil, in quest of nourishment
from the earth, so do the plants direct their growth, and turn
the upper disk of their green leaves to the light, and to the
light alone.

[*Note* 13. *p.* 230.]

This theory, so opposite, as Dr. Black observes, to the
theory of Stahl, is not so recent as is generally imagined.   It
was seen, in all its extent and importance, by Dr. Robert
Hooke, one of the greatest geniuses and most ardent inquirers
into the operations of nature, who figured during the latter
half of the seventeenth century, a period full of great disco-
veries.

Dr. Hooke proposed this theory in considerable detail in
his *Micrographia*, published in 1665 ; (*see p.* 103.) ; and in

his *Lampas*, published in 1676: and he makes it an important doctrine in his treatise on Comets, and in many passages of his Cutlerian Lectures. He promises to take it into serious consideration, and to publish a full exhibition of it. The allusions made to it in his Lectures make it evident that he had continued to make some desultory additions to his first conceptions. His *Lampas* contains a most accurate explanation of flame, which cannot be surpassed by any performance of the present day.

In the Micrographia he states the theory in the following words:

1. The air in which we live, and breathe, and move, and which encompasses and cherishes all bodies, is the universal solvent of all sulphurous (synonymous, at that time, with inflammable) bodies.

2. This action it performs not till the body be sufficiently heated, as we observe in other solutions.

3. *This action of dissolution produces the great heat which we call fire.*

4. It acts with such violence as to agitate the particles of the diaphnaous body, air, and to produce that elastic pulse called light. (*See his own hypothesis concerning the propagation of light.*)

5. This action, or dissolution of inflammable bodies, is performed by *a substance inherent in, and mixed with the air, that is like, if not the very same, with that which is fixed in saltpetre.*

6. In this dissolution of bodies by the air, a part of the body, uniting with the air, is dissolved or turned into air, and escapes and flies about.

7. As one part is thus turned into air, so another is mixed with it, but forms a coagulum, or precipitation, some of which is so light as to be carried away with the air, while other grosser and heavier matters remain behind, &c. &c. This latter article is frequently employed in other parts of his writings, and is sometimes called a grosser *compound*, mixed with matters terrene, and originally insoluble in air, and incombustible.

Can any thing more be wanting to prove that this is the same with the modern theory of combustion? Nothing but to shew that this coagulum contained the air which had formed it, by shewing an increase of its weight, or by separating it again. But the eager mind of Hooke, attracted by every appearance of novelty, was satisfied with the general notion of a great subject, and immediately quitted it in chase of some other interesting object. Had he not been thus led off by a new pursuit, this wonderful man would not only have anticipated, put completed many of the great discoveries of the last century. It was a bold conception, and only a vigorous mind could entertain it for a moment, that the vast heat of combustion was contained in a few grains of air. Yet this was his opinion, as appears by the explanation which he gives, in various meetings of the Royal Society, and in his lectur.s on comets, of the deflagration of combustible bodies with saltpetre, and of fiery motion.

In the treatise called Lampas, he observes that this his treatise, published eleven years before, had been very favourably received, and that he had not seen any valid objection offered to it. It was in this interval that Dr. Mayhow, at Oxford, published his Book *De Sale Nitro, et Spiritu Nitro-aërco*, in which he holds precisely the same doctrine. But his exhibition of it is obscure, complicated, and wavering, mixed with much mechanical nonsense, of wedges, and darts, and motions, &c. according to the fashion of the times. Hooke's conception of the subject, on the contrary, is clear, simple, and steady. The only addition made by Mayhow are some observations on the increase of weight observed in the preparation of diaphoretic antimony, &c. Hooke, explaining at a meeting of the Royal Society, some tricks of the plumber's workmen, who called the litharge which formed on the surface of melted lead dross, and took it with them as their perquisite, says expressly, that they can make dross of the whole, and that it is more than the lead by all the air which was its menstruum. But Mayhow wrote on the subject expressly, and it appears in the title of his book. He is remembered, while Hooke is forgotten; because no one would think

of looking into the Micrographia for chemical information. The theory comes in by chance, to explain the indestructibility of charcoal in close vessels by heat. Mayhow also made many very ingenious experiments on the air which had contributed to inflammation, and has anticipated both the manipulations and the discoveries of modern pneumatic chemistry.

I do not know a more unaccountable thing in the history of science, than the total oblivion of this theory of Dr. Hooke, so clearly expressed, and so likely to catch attention. No notice, that I know of, had been taken of it, till I mentioned it in the supplement to the Cyclopædia Britannica, in the sketch given of Dr. Herscheil's discovery of the radiation of solar heat, in the article *Thermometrical Spectrum.* I made the observation in 1798, having then got a copy of Hooke's Lampas, and other tracts, which are as curious as they are rare. And I then requested Sir Joseph Banks to order a search among Dr. Hooke's papers, in the possession of the Royal Society. I am persuaded that many of his speculations on this subject will be found among them; and I earnestly request an inspection of them to be made.

I trust that the reader is not displeased with this my endeavour to do justice to the well founded claim of our illustrious countryman. Had Dr. Hooke's fortune allowed him to multiply experiments, to associate in his labours a knot of eminent assistants, and to form *a party* in the Society, ready at all times to meet and to document every occasional discovery, I make no doubt but that he would have stood next to Newton in philosophical rank : but " his lot forbade." Other *eminent* men would willingly have benefited by his genius, but would not be his seconds. They used him extremely ill; and this increased his distrust and jealousy. He does not seem to have had the least ambition to head a sect, although he sometimes boasts too much of his own inventions. Not incited by this ambition, all his efforts were solitary,....merely to quench his own ardent thirst for knowledge. His temper, soured by ill usage, and by the infirmities of a most feeble and sickly constitution, made him quarrel with all his friends in the Society ; and, in so doing, he was guilty of some in-

gratitude ; for he had been encouraged, protected, and even borne with, with some tenderness, on account of his genius and his infirmities. But all these things contributed to the neglect of his writings, and this took away the chief incitement to the prosecution of this ingenious theory. He did not, however, altogether give it up ; for some of his occasional employments of it allude to recent observations. I am per- suaded that valuable thoughts on this subject are still to be found in the chest of papers committed to the keeping of the Society by Mr. Waller, after he had extracted from them what he has published as Dr. Hooke's posthumous works. Dr. Hooke died in peace ; and his memory was honoured by his colleagues in the Society. Mr. Lavoisier fell a sacrifice to the ambition of those whom he had associated with him in his researches, all of whom either were at the time of his death, or became, in a few months, ministers, or generals, or persons in high public functions. France, indebted to this man for the only honour that it has acquired during the event- ful period of the revolution, condemned him to death, *for putting water into the tobacco, and other things hurtful to the health of the citizens.*

........Quis, talia fando,
Temperet a lachrymis ?........

[*Note* 14. *p.* 232.]

When the dish is filled to the brim, so that the air may have free access the burning fuel, and we take care, at the same time, to hinder it from being thrown into eddies, by setting a glass cylinder round it, at a small distance, allowing the air to get in between it and the dish, we shall observe the flame occupy the whole circumference of the dish. But if a small bit of paper be lying on the middle of the spirits, or even supported at a small height above it, we see that it is not burnt, though surrounded by flame. It is only at the outer surface of the ascending vapour, that the inflammation is going on. What is within cannot burn ; because there is no air in con- tact with it. If we hold a glass plate above it, so as to hinder

the flame from rising high, and we then look down through the glass, we shall see that none of the internal part is burning. But if we introduce a pipe through this film of flame to the middle, we shall see a stream of flame arise from the end of this pipe, and we are ready to believe, that there is a stream of inflammable matter coming from the pipe. But it is only air which comes out, and kindles what it touches. This flaming stream from the pipe is also only a cylindrical film of flame like the other. These observations and remarks enable us to explain the upright and tapering form of all flames. Flame rises, because it is a hot vapour, which expands the contiguous air, which therefore rises by its specific levity, and carries up with it this shining vapour. Its form is tapering, because it is luminous only where it is really burning, that is, in contact with the air. The exterior stratum is burnt before it gets far up. The next rises till it overtops the outer one, now burnt; and it burns in its turn, getting a little higher before it is consumed. The third stratum must get above the second, before it begin to burn; and it adds a little more to the height of the flame : And thus the strata burn at top, in succession, and the centre one closes the pyramid. Dr. Hooke gives a most accurate description and explanation of all the phenomena of flame, illustrated by a figure, in the *Lampas*, p. 5. proving it to be a shining film of inflamed vapour.

In consequence of this superficial constitution of a flame, whenever we cover it with any thing which prevents the access of fresh air, it is instantly extinguished. But an inflammable body that is solid, and fixed in a great heat, may be superficially extinguished in this way. But if the cover be taken off immediately, the surface, which appears black at first, soon grows red again, by the heat of the interior parts, and the combustion recommences.

Combustible bodies which have an intermediate degree of volatility burn with a flame more or less violent. But when a considerable portion is flaming at once, it rarely happens that the combustion is complete in the interior parts of the flame. Much of this vapour is only scorched, or charred, and is hurried away in this condition by the current of air ge-

nerated by the heat.. This charred vapour is what we call
smoke, and *soot ;* both of which are still highly inflammable.
The sure way of producing a more complete inflammation is
to prevent a great quantity of surface from flaming at once.
Thus, a very small wick is found to burn without smoke or
soot ; and Argand's lamp, which admits a current of air into
the centre of the flame, produces the same effect in an eminent
degree. I employed such a lamp in 1767-8. It consisted of
two concentric circles of wicks of rush pith stuck on pins. The
air came up in the centre, and between the circles. This was
the lamp of a distilling apparatus, and stood on the laboratory
table of Glasgow college for two years. Quitting all chemi-
cal pursuits in 1769, I thought no more of it.

[*Note* 15. *p.* 254.]

For Sir Isaac Newton, in stating this violent conflict of
forces, in order to account for the turbulent motions really ob-
served, did not proceed, 'as many of his followers have done,
and *suppose* the force to be great, because he had a violent mo-
tion to account for. He first prepares the way, by proving,
that, in the corpuscular actions of bodies, we *observe* forces
which are inconceivably great. He shews that the sum total
of the force with which one grain of glass attracts the light
which it refracts, is not less than the force with which a wire
would be stretched by a hundred thousand millions of millions
of pounds of matter hanging by it. This is surely competent
to the production of the most violent explosion that chemistry
presents ts our wondering view. Observe too, that it did not
escape the remark of Newton, that *this identical force* is con-
cerned in all the most violent effects of chemical mixture. They
are accompanied by the extrications of heat and of light. When
we come to consider some of these motions in detail, we shall
be better able to appreciate the wonderful sagacity and pene-
tration of this extraordinary man. We shall see with dis-
tinctness why argentum and mercurius fulminans, while they
explode with a force certainly greater than that of gun-
powder, cannot propel a bullet with half the force which it can
impart.

[*Note* 16. *p.* 255.]

It will prevent many mistakes if we adhere strictly to this meaning of the term ; if we consider it merely as an abbreviated way of saying that the particles of bodies do really thus come together. We shall find abundant employment in ascertaining with precision all the cases in which this action is to be observed, the limits to its generality, and the peculiar modifications of it, which distinguish certain classes of this phenomenon, and the cases or substances which exhibit these distinctive modifications ;....and this employment will advance our chemical knowledge incomparably more than any speculation about its cause. At the same time, I see some advantages attending the use of the term *chemical affinity*. It distinguishes very compendiously the phenomena of combination (which are the chief objects of chemistry) from the phenomena of cohesion, adhesion, capillary attraction, &c. which are affected by forces perfectly similar, but are not so characteristically chemical. But further,....attraction surely is the drawing one thing toward another. Now there are many instances where the bulk of the compound exceeds the sum of the bulks of the ingredients. The particles, therefore, have not been drawn towards each other. Affinity does not, in general, imply any similarity. When it is so used, we are sensible of some degree of figurativeness or metaphor.

But whether we employ the word attraction or affinity, we must be careful to attach no other idea to it but that of a fact, a determination of each particle of a certain substance to unite chemically with each particle of another certain substance, so as to constitute particles of a compound of both. We run great risk of mistake, if we adopt any notion of the mode of action by which this union is effected. The late discoveries of the chemical phenomena called *galvanic*, seem to derange all our notions of the immediate agent in chemical union or separation.

[*Note* 17. *p.* 255.]

I think that it will be of service to remark, once for all, that
we have no denominations for these forces which we conceive
as the immediate agents in natural phenomena, except the
phenomena themselves, which we conceive them to produce.
We call them the force of *gravity*, *cohesion*, *refraction*, &c.
In a few cases, we name them by the substances in which they
seem to reside, as *electricity*, *magnetism*. This circumstance
alone shews that we have no direct knowledge of the, nature
and mode of action of those forces ; they are not objects of our
observation ; and our knowledge of their supposed effects
that is, our knowledge of the phenomena, is all that we shall
ever acquire of the forces or supposed causes.   When we go,
or think that we go, a little farther, and call the force of cohe-
sion an *attraction*, we mean nothing more than that the effect
resembles the effect of a real attraction, or pulling a thing to-
ward us with our hand.

[*Note* 18. *p.* 257.]

In employing the words *stronger* and *weaker*, as expressing
qualities of the chemical attractions, we must be careful not to
use them precisely in the same way as in mechanics.   They
must be used merely as an expression of *prevalence*, but not as
expressing a measurable quantity, to which we give the name
of *intensity*, otherwise we shall run the risk of mistake in our
conceptions of those quantities. Aquafortis separates the parts
of iron ; and we express this also, by saying that the attraction
of iron for aquafortis is stronger than its cohesive attraction
for its own parts. We say the same thing of potash,....and we
say that the attraction of aquafortis for potash is stronger than
its attraction for iron, because potash separates aquafortis from
iron.   These expressions might oblige us to say that the co-
hesive force of iron is less than that of potash, did we not know
the contrary. The *prevalence* is all that we should conceive to
be expressed ; and this rather arises from the peculiar mode
of action, than from the greater intensity. Perhaps there is

nothing in many of those chemical changes analogous to what we call resistance and counteraction in mechanics. We see, indeed, some cases in which we cannot doubt it, as in the prevention of vaporisation (and perhaps of congelation in some cases) by external pressure.

[*Note* 19. *p*. 260.]

Dr. Black sometimes explained the effect of stagnation, and the propriety of agitating the mixture, at length, but has not committed his view of the matter to writing. I recollect distinctly the explanation which he gave in 1765 at Glasgow. A tall beer glass being filled with water, a long funnel was put into it, whose pipe reached to the bottom, and as much of a saturated and deep-coloured solution of blue vitriol was poured in, as occupied about an inch at the bottom of the glass. It was set on a bracket, having a white paper behind it. This was done some days before this lecture: and the students were made to observe how the colour slowly ascended upwards, growing fainter and fainter as it was farther up the glass. In this lecture Dr. Black observed, that this gradation of colour shewed that the different horizontal strata of the fluid were in different states of saturation, and because the colour did not change by sensible steps, but gradually, the difference between two contiguous strata in respect of saturation was infinitely small. Therefore, since the attractive force of a stratum was so much the greater, as it contained less of the salt, and as the adjoining stratum contained more, it must follow that, in any horizontal section, the force drawing the salt upward must be extremely small, and the motion extremely slow; and that the only method to make it proceed faster, is to bring the parts which contain least salt into contact with those which contain most. As a proof of his doctrine, he kept the glass clear of all disturbances, on the bracket; and three months after, the colour had not risen three inches. Indeed it did not reach near the top after three years. Dr. Black explained the slow communication of heat along the

parts of solid bodies, and the effects of stagnation in the atmos-
phere, in the same manner.

[*Note* 20. *p.* 263.]

It may be said here, that this manner of generalising the
events is gratuitous, and that we have no other test of the pro-
portional strength of the chemical forces concerned in these
phenomena but the phenomena themselves. We call that a
stronger force which separates two ingredients united by the
force which we term the weaker. It must be acknowledged,
that this generally has been the evidence on which the propor-
tion of the attractions has been established ; and it is legiti-
mate, and is equally true in mechanics and every department
of physics. But it is not our sole authority, even in the che-
mical actions of bodies. It is perhaps true without exception,
that, when these different combinations (which may be thus
changed in succession by a third body) can also be destroyed
by the varied force of one agent, such as heat, we shall find
that those ingredients which manifest the strongest *elective* at-
tractions really require the greatest exertions of this general
agent to separate them. The exceptions seem, at first, to be
numerous and remarkable ; but when the co-operation of dif-
ferent degrees of fluidity and volatility are considered, almost
the whole of the exceptions are found to come under the ge-
neral rule.

[*Note* 21. *p.* 263.]

When Sir Isaac Newton took that cursory view of chemical
composition and decomposition which he has expressed in
the queries annexed to his Optics, he saw well how vain an
attempt it would prove, to reason minutely on those forces, on
mechanical principles. For he had already found, that his
own utmost efforts could only approximate to the solution of
the problem of three particles acting on one another. Nor
have the united efforts of all the mathematicians of Europe,
since his time, produced an accurate solution of this celebra-
ted problem. What then can we hope for, when we must

consider at once the combined action of myriads of particles? He contented himself, therefore, with considering the phenomenon of chemical combination as the only accessible mark of the acting force, and recommended the study and comparison of these as the only means of improving the science. It was, as yet, far removed from first principles; and the simplest phenomenon presented to our view is a very complex event. The only proper method of proceeding is to compare them, and class them according to their extent and generality, in the hopes of finding points of resemblance, which would form a law still more general, and therefore nearer to the first principles of the science. The separation by a third body is a very general law, and it did not appear necessary, or even proper, to speculate on its cause.

Notwithstanding the eager endeavours of his followers to account for the separation, or for the extreme rarity of a combination of three ingredients in a series of elective attractions, no satisfactory explanation has yet been obtained. Few chemists were mechanicians: and those who were eminent mathematical philosophers, such as Keil, Bernouilli, and a few others, were but sorry chemists. Boscovich is the first, who, by considering corpuscular force in the most general and abstract manner, has shewn how the action of two particles may be totally annihilated by the action of a third, and this, even although the action of this third on either of the other two, be in any degree *weaker* than the mutual action of those two. He shews that this will depend as much on the kind, as on the general magnitude or intensity of the force.

It may perhaps assist the imagination a little, to point out some examples of the same kind which we can explain to a certain degree. When a piece of iron is hanging by the north pole of a magnet, if we present the south pole of another magnet to the place of junction, the iron immediately drops. Moreover, if we hang to a magnetic pole a piece of iron as large as it can carry, and then attempt to hang another such piece beside it, the first will certainly drop from it. Again, we know that a metal plate, negatively electrified, attracts a rod of metal very strongly. We know that a plate positively elec-

trified does the same. Yet we also know, that a charged plate
of glass, one side of which is *most highly* positive and the
other as highly negative, exerts scarcely any attraction on the
metal rod. And we understand in a considerable degree how
these things happen. A still more apposite illustration may
be had by the following curious phenomenon in magnets,
which had not been noticed till I mentioned it in the article
Magnetism, of the Cyclop. Britan. § 29.
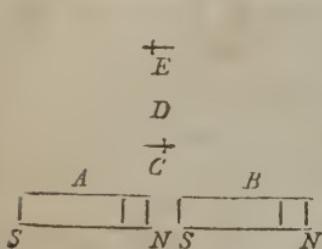
Having laid two good magnets, A, B,
of equal power, on the table, with the
north pole of A near the south pole of
B, and the two magnets forming a
straight line, take a very small compass
needle, such as are frequently mount-
ed for wearing on a watch chain, and
place it at C. The needle will immediately place itself parallel
to the magnets A and B, and its poles will be in the same di-
rection with theirs. Draw it out perpendicularly from the two
magnets, and you will find its polarity gradually diminish, as
you may perceive by its vibrations growing slower. When at
a certain distance, at D, its virtue seems entirely gone; for it
will remain in any position that is given it. Draw it still far-
ther off, and you will perceive its virtue returning again, but
its polarity is reversed. At E, as far from D as D is from
C, it will stand in the position contrary to that of C.

While the needle is at E, gently draw the magnets farther
asunder, both equally from the middle point between them,
and keeping them still in the same line. When at a certain
distance, the virtue of E is again destroyed, and it will retain
any position. And, on separating them still farther, E reco-
vers the same polarity that it had in C.

From this experiment, we see how much the mutual rela-
tions of particles endowed with powers like polarity, de-
pend on the precise position, and the peculiar constitution
of the subject which affects them. If, instead of one little
magnetic needle which we place successively at C, D, and E,
we employ several, turning on pins stuck into little pedestals,
we shall observe them all affected, and attracting each other

by the ir nearest poles ; and when the distance of **A** and **B** is properly changed, the virtue of the little needles is also changed, every pole repelling what it formerly attracted.

[*Note* 22. *p.* 266.]

In all these cases, the progress of chemical science has been greatly obstructed by the attempts to reason from higher principles. We know, and ever shall know, so very little about the mechanism of chemical attraction, that we can only frame hypotheses, and then reason from such hypotheses. No accession of knowledge is obtained by this procedure. For our hypothesis, in order to have any influence on our mind, must be founded on a certain extent of resemblance to the phenomena ; and its whole value depends on that resemblance. We have the phenomena themselves ; and *their* coincidence is our *only* general law. The hypothesis is only a fanciful picture of this law : it is therefore useless. It does not support the phenomena ; but it is founded on them, if it be good for any thing. In the present case Mr. Berthollet has wisely avoided reasoning from any supposed properties of chemical attraction ; and deduces all his general laws from extensive coincidences of facts.

[*Note* 23. *p.* 266.]

Besides the authors mentioned by **Dr. Black**, many useful remarks and general views may be gathered from the tables of other eminent chemists. *Marherr de affinitatibus Corporum, Viennæ,* 1762,....and particularly *Wenzel's Doctrina Affinitatum, Dresden,* 1777,....and *Kirwan on the specific Gravities and attractive Powers of different Substances, London,* 1781,....deserve a careful perusal. The table in *Gren's Chemistry* includes almost every article, and every particular view that has been taken of the subject. *Wenzel* endeavours to prove, that all the distinguishing attractions of different substances are only modifications of the general attraction of gravitation ; and are modifications arising from the figure and structure of the ultimate particles of each substance. But Wenzel had not the

mathematical knowledge necessary for such a task. Other authors have had the same notion. And, no doubt, the distinguishing actions of different substances may have this origin,....that is, may depend on the figure and structure of the particles, although the primary atoms, of which these consist, have but one law of action. This is the great principle of Abbé Boscovich's theory. But I think it strictly demonstrable that no figure or structure will constitute such particles as we observe, if the action of the primary atoms be an attraction in the simple ratio of the square of the distance inversely : and I am certain that no repelling particles, or repelling body, can ever arise in this way.

[*Note* 24. *p.* 268.]

This most interesting law of chemical combination may be conceived, if not explained, in the following manner :
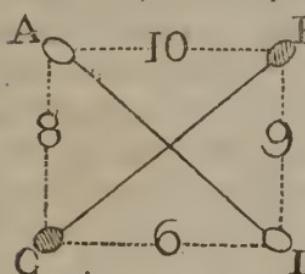
When a compound consisting of the ingredients A and B is mixed with another, of which the ingredients are C and D, there are two affinities or forces supposed to be in action ; one connecting A with B, and the other connecting C with D.

But let us also suppose that A has an affinity to C, or attracts it as well as B, and that B also attracts D. Then, in the mixture we have two other forces, one connecting A with C, and the other connecting B with D. We may express these forces by the symbols $A \times B$, $C \times D$, $A \times C$, $B \times D$.

The two first may be called *maintaining forces*, because they tend to keep things in their present condition. The forces $A \times C$ and $B \times D$ may be called *divellent*, because they are conceived to have a tendency to separate A from B, and C from D. This supposition is founded on the authority of the general fact of separation of two ingredients, by presenting a third substance which has affinity to one of them.

From this manner of conceiving the subject, it is inferred, that if the sum of the forces $A \times C$ and $B \times D$, exceed that of

the forces A × B and C × D, we shall have a double exchange, and two new compounds. This may happen, therefore, although the attraction of A for B should exceed its attraction for C, provided it do not exceed it so much as the attraction of D for B exceeds its attraction for C.

Since it is a general fact in chemistry, that substances act most powerfully in their simple state, it may appear strange that C, when already united with D, should overcome the stronger attraction of A for B, which it cannot do alone. This shews us, by the way, that the *prevalence* in chemical action depends rather on the manner and concomitant circumstances of the action, than on the measurable intensity or magnitude of a particular force. Dr. Cullen used to represent this *chemical fact* by the diagram in the margin, where the numbers placed between the substances express the supposed attractive forces exerted between the substances. This diagram suggests the notion of bodies attached to the ends of two rods or levers, AD and BC, moveable round their intersection, E. Were this the case, it is certain that the attraction of A for C, and of B for D, tends to separate A from B, and C from D: and what is asserted above will happen,....the levers will close between AC and BD; and A will apply itself to C, and B to D. Dr. Black first employed this diagram: but he gave it up, because it suggested a notion not chemical, but mechanical. Levers can have no place here. It suggests also an erroneous notion. The levers produce the effect, only in consequence of a connection which they establish between A and D, and between B and C. Now, in by far the greatest number of cases in which a double exchange is observed, we know of no such connection. He used to express the cases more in the style of chemical phenomena, by saying, that, in order to have a double exchange, the *partiality* of D for B must exceed that of A for B. But still this is merely a figurative expression of an unknown cause.

It is not easy to conceive any mode of operation which will clearly produce the observed effect. It confirms, I think, the considerations mentioned in note 21, p. 263; and it re-

ceives some illustration from the magnetical phenomena mentioned there.

When we say that a double exchange happens when $A \times C + B \times D$ exceeds $A \times B + C \times D$, we seem to say something like instruction. But, in truth, it is only an inference of this greater partiality from the observed effect. We have generally no other argument for saying that the excess of D's attraction for B above its attraction for C, is greater than the excess of A's attraction for B above its attraction for C. The fact is that we have inferred all these partialities only from the observed double exchanges.

Still, however, these double affinities are among the most interesting chemical laws, and of immense use for operating changes which are otherwise impossible. Therefore a copious collection of the cases in which they are observed, will be a vast accession to chemical science, especially if judiciously arranged.

Did we know every case in which one body can detach another, so as to make up a table of single affinities complete, we might then, by comparing them in parcels of four, tell beforehand whether there will be a double exchange : but our knowledge is not yet so far advanced.

No where are the double exchanges so frequent as in the spontaneous fermentations of animal and vegetable juices. A fertile imagination, by employing four agents in this way, and assigning a convenient order of their action, may bring out almost any ultimate result he pleases. It is, therefore, a delicate part of the science. There are some very judicious observations on the marks and characters of double affinities, in Dr. Lubbock's inaugural dissertation, *De Principio Sorbili*, which a philosophical chemist should be very clearly acquainted with ; otherwise he is liable to many great mistakes.

We must multiply, as much as possible, the cases of double exchange by experiment; and we must apply to each case numbers which will agree with the observed prevalence. We must correct and recorrect those numbers, so that the same number may always belong to the same substance, and yet suit its place in every case where that substance is one ingre-

dient in a double exchange. This is a long and difficult task; but till it be accomplished, there is no certainty in the ingenious explanations given of many complex phenomena; especially where a change of temperature makes a change in the chemical attractions.

[*Note* 25. *p*. 290.]

It may be carried much farther than this, by carefully preventing dissipation. We see what considerable heats are produced in the common garden glasses, and in greenhouses, merely by preventing the air from absorbing it and carrying it off. Mr. Saussure *(Voyage sur les Alpes,* II. 932.*)* made a little box, lined with fine dry cork, whose surface was nicely and uniformly charred, to make it black, and also more spongy and unfit for conducting heat. It was covered by a very fine and thin plate of glass. A thermometer being laid on the bottom, and the box set in the sun's ray's, when the temperature of the surrounding air was 75° Fahrenheit, the inclosed thermometer rose to 201° in a few minutes. The account is somewhat equivocal, in the way of expressing the change on the thermometer; and I see some foreign writers conclude that it rose to 229°, 17° above boiling water.

I c   apparatus of this kind, employing three very thin   flint-glass, which transmits much more heat (acco   ng   Herschell's experiments) than any other species of glass. They were nearly of similar shapes, arched above: and there was about one-third of an inch of an interval between them. They were set on a cork base, prepared like Mr. Saussure's, and set on down, contained in a pasteboard cylinder. With this apparatus I have often, in a clear summer day, raised the thermometer to 230°   d once to 237° of Fahrenheit's scale. Even when set b   bright fire, it raised the thermometer to a boiling   t. When I took the apparatus into a damp cellar, before I set the glasses in their place, so that the air between them was damp, I found that when exposed to the same degree of the sun's light, I never could raise the thermometer above 208°;